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# Edward Bradford Titchener
## 1867-1927

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After thirty-five years of continuous service to psychology at Cornell University, Edward Bradford Titchener, the dean of experimental psychology in America,[1] died unexpectedly in Ithaca, New York, on August 3rd, 1927, at the age of sixty. The death of no other psychologist could so alter the psychological picture in America. Not only was he unique among American psychologists as a personality and in his scientific attitude, but he was a cardinal point in the national systematic orientation. The clear-cut opposition between behaviorism and its allies, on the one hand, and something else, on the other, remains clear only when the opposition is between behaviorism and Titchener, mental tests and Titchener, or applied psychology and Titchener. His death thus, in a sense, creates a classificatory chaos in American systematic psychology.[2]

Titchener was born on January 11, 1867, at Chichester, England, an old Roman town with walls still standing, about seventy miles south of London and not far from the coast. There is an insistent historicity about this little town that accords well with the historical cast of Titchener's mind in adulthood.[3] The Titcheners were an old Chichester family. In 1532 John Tychenor was the head master of the Prebendal School in Chichester, which our Titchener attended in boyhood. William Titchener, the great-grandfather, was mayor of Chichester in 1832, and lent

[1] E. B. Holt's phrase, in reviewing Titchener's *Text-book*, was "the dean of American empirical psychology," *Psychol. Bull.*, 8, 1911, 25; but Titchener's name is since connected with the creation of an antithesis between 'empirical' psychology (Brentano) and 'experimental' psychology (Wundt, hence himself).

[2] This biographical sketch is indebted throughout to many of Titchener's friends and colleagues who have been glad at this time to revive memories and to impart information. Besides Mrs. Titchener, I should mention especially Professors Frank Angell, Warren, Pillsbury, and Dallenbach.

[3] He himself once suggested this relationship.

money to the city in return for which he received a lien on the regalia of the city. His son Edward (the grandfather) was a distinguished lawyer, according to a long list of appointments which he held. He is said to have run through several fortunes and to have died relatively poor, having been obliged to move from a residence in an old abbey, Greyling Wells, to a smaller house. He had two sons, William and John; and John was the father of our Titchener. John was the second son, but anyway, there was no longer a fortune for him to inherit. He went on an adventure to America at the time of the Civil War, and returned to England, after fighting with the Confederate army. In 1866, he married Alice Field Habin of Kingsham, which is not far from Chichester, and in 1867 Edward Bradford was born. John Bradford died not many years afterward while still in his thirties. Edward Bradford Titchener therefore had his own way to make. There was no parental assistance forthcoming for him; he had, so far as his family was concerned, simply the consciousness of a very respectable descent.[4]

When he was about fourteen he went on a scholarship to Malvern College, an important English public-school, then about twenty years old. The college is in Worcestershire, over a hundred miles from London and very near Wales. There he spent four years.

After Malvern he went to Oxford in 1885, and became a member of Brasenose College. For four years he was senior scholar of Brasenose in classics and philosophy and senior Hulmian 'exhibitioner' (a term applied to the holder of what, in America, is called a scholarship).[5] In his fifth year he became a research student of physiology under Burdon-Sanderson, either because of the attraction of the subject or of the man, or perhaps because of both. He always expressed a great admiration for Burdon-Sanderson and acknowledged an intellectual debt to him. His first book he dedicated to him. He received the A.B. degree from Oxford in 1890, when he was twenty-three years old.

The conjunction of philosophy and physiology in Germany had produced physiological psychology and it is not strange that the combination of these two interests at Oxford should have led Titchener toward the new psychology. It was really quite new, much newer than the thirty years since Fechner's *Elemente* might lead one to suspect. Wundt's laboratory at Leip-

[4]These things seem to me important to mention, not because of biographical convention but because they were important in Titchener's mind. The strong family tradition with its roots in Fnglish soil may be a partial explanation for the surprising fact that Titchener could spend the last thirty-five years of his life in America without once visiting England, and yet remain a staunch British subject without apparently a thought of becoming an American citizen. During the War his "we" meant Great Britain and his "you" America.

[5]An exhibitioner is appointed in part because of need of financial assistance. The benefaction was named for its founder, William Hulme (1691).

zig had been in existence only eleven years. On the other hand, interest was growing rapidly. Wundt's *Philosophische Studien* was in its sixth volume; the *Zeitschrift für Psychologie* had just been begun; Stumpf was completing the *Tonpsychologie;* Ebbinghaus' experiments on memory were five years old; G. E. Müller had begun on psychophysics; there was plenty to read in German. America was beginning to follow suit. Hall had opened the first formal laboratory at Hopkins, based on the German model, had gone to Clark and had begun another there. Cattell, back from Germany, was experimenting at Pennsylvania. Three other American laboratories had started and three more were about to be gotten under way. *The American Journal of Psychology* had reached its third volume. Ladd's *Physiological Psychology* was three years old, and James' *Principles* was to appear in the fall. The year before there had been the first International Congress of Psychology at Paris with one hundred and twenty psychologists in attendance. England was slow to learn from Germany, but the founding of *Mind* by Bain in 1876 had been in a sense a recognition of the birth of a new psychology. It was at such a time that Titchener, trained in philosophy and physiology, went to Wundt at Leipzig to study experimental psychology.

At Leipzig he found Külpe, as *Dozent*, Meumann, Kirschmann, Kämpfe, and Pace, Scripture and Frank Angell from America. This was the beginning of a lifelong friendship and mutual regard between Titchener and Angell, based upon similar tastes and Angell's admiration for Titchener's "impersonal frankness." The second year Pace, Scripture, and Angell left, and Witmer and Warren came. Titchener roomed with Meumann. Like the graduate students of any laboratory the world over, this group was much together in informal psychological discussion. Külpe was planning and writing his *Grundriss* which he published in 1893 and which Titchener translated into English in 1895. The thought was somewhat communal, and Titchener published in 1896 his *Outlines* on a similar plan. Some of the discussion, I have been told, hinged upon the fundamental methodological issue. Wundt had separated psychology and physics by reference to immediate and mediate experience, but younger men are always anxious to improve upon the master. Mach's *Analyse* had appeared in 1885; Avenarius had brought out the first volume of the *Kritik* in 1888 and the second in 1890. We all know that these books formed the basis of Külpe's and Titchener's definition of psychology. Külpe, as the more philosophical of the two, is said to have been especially concerned with Avenarius; Titchener, perhaps already turning from philosophy to science, preferred Mach, for whom his enthusiasm was never dampened.

The contact with Wundt was not personally intimate. Wundt was thirty-five years older than Titchener. He had not yet reached the height of his fame, but he was already the great man, expecting and receiving deference and homage, and capable of becoming "white and trembling with anger when some of his 'Zuhörer' . . . . slipped out of a lecture on philosophy shortly before its close." With the graduate students "he dealt . . . . somewhat autocratically, and prescribed the lines and methods of research . . . . . minutely."[6] Probably Titchener's work on the *Erkennungsact*[7] and the doctoral dissertation, *Über binoculare Wirkungen monocularer Reize*,[8] were arbitrarily assigned in this way.

But if Titchener's intercourse with Wundt was not intimate personally, it was nevertheless both intimate and effective academically. The similarity between Titchener and Wundt may be partly merely a coincidence that they were similar personalities, though one was an Englishman and the other a German. Nevertheless this fact is not enough to explain all of the many overt items of resemblance. At least a dozen attitudes of Titchener's he got from Wundt.[9] There is his own autocracy in the Cornell Laboratory, not only in the matter of theses, less obvious perhaps than Wundt's, but, supported by his dominating personality, equally effective.[10] There is his persistent assumption that all the men of the Cornell Laboratory represent a single intellectual endeavor, opposed to an uninitiated outer world. There are both his tolerance in self-criticism and his inflexibility in polemics. There is his constant effort to demonstrate that psychology is a science. There is his consequent belief in formal psychological apparatus, and the need of 'building up' a laboratory by the collection of standardized pieces. There is his conviction that the elementary lectures should be accompanied by experimental demonstration, and also his habit (but here he excelled Wundt) of dramatizing his lectures. One device was his invariable custom of lecturing in an Oxford master's gown: "It confers the right to be dogmatic," he said. Another was the tradition under which the members of his staff have always attended the elementary lectures—and listened often, let it be said, with suppressed excitement to hear how, after some new discovery at Cornell, he would alter the

[6] For the picture of Wundt in 1890-92, see the sketches by Pace, Frank Angell, and Warren (from the last two I have quoted) and Titchener's letter written in 1890, *Psychol. Rev.*, 28, 1921, 159-169. Students of Titchener's character may well read these and the adjacent accounts of Wundt, since there can be no doubt that Leipsig formed the pattern for Cornell.

[7] *Philos. Stud.*, 8, 1892, 138-144.

[8] *Ibid.*, 231-310. The offprint with the short *Vita* is also extant.

[9] The concluding paragraphs of this biography discuss other more subtle ways in which the Wundtian impress upon Titchener appeared.

[10] American students could not of course accept Titchener entirely without conflict. He excited both fear and affection (or admiration) in his associates.

familiar treatment of the subject. Could the picture be more like Leipzig and less American or English?[11]

Actually then there were only two years of Wundt. In 1892, when twenty-five years old, Titchener received the degree of Doctor of Philosophy and returned to England. Here the record shows that he was an extension lecturer in biology at Oxford for a few months, a very natural arrangement in view of his relation to Burdon-Sanderson and of the fact that he had already since 1889 published ten articles in general biology, all of them in *Nature*. Titchener would have liked to stay on at Oxford, but Oxford has never, then or now, adequately recognized psychology, and experimental psychology as a science would have had there a particularly hard row to hoe.[12] At any rate on leaving Leipzig he had already agreed to go to America.

Frank Angell, his Leipzig friend of 1890-91, had gone back to America to Cornell to open a psychological laboratory initiated by a benefaction that phrenology is said to have inspired. After his first year at Cornell he was called, at its inauguration, to Stanford University, and he recommended Titchener for his Cornell post. "In those days a laboratory was a laboratory," Titchener has often said; and this young laboratory psychologist, still only twenty-five years old, hurried to Ithaca, New York, with its laboratory, as an assistant professor of psychology. He never left Cornell; and within psychology Cornell and Titchener have become almost interchangeable words.

Ithaca was certainly a remarkable change from Oxford and Leipzig. The scenic beauty of the Cornell campus, between two deep gorges, was then, except for mud, as it is now; but the university, architecturally at least, was raw. Nevertheless Titchener had his laboratory. The first years were strenuous ones. The six-room laboratory in White Hall had to be equipped with bought standard pieces and with home-made apparatus.[13] He married in 1894, Sophie K. Bedlow of Portland, Maine, and she helped him greatly in the Laboratory and later with drawings for the books. He may have been discouraged, for in 1893 he applied for the post at Toronto, and his effort to leave so soon after he had come was not looked upon with favor by the University.[14]

He plunged immediately into scientific work and writing. His list of publications contains sixty-two articles, long and

[11]Titchener was often mistaken by those who knew him slightly, or who knew only about him, for a German—once even by a younger English psychologist.

[12]It was only in 1882 that the physiological laboratory was founded at Oxford for Burdon-Sanderson against opposition of the most violent kind, first against the building, and three years later against its equipment.

[13]Titchener brought with him, all the way from Leipzig, Wundt's original demonstrational tachistoscope, a massive wooden affair over seven feet high and weighing about fifty pounds, with a heavy wooden shutter, whose drop on felt can be heard far down the hall.

[14]Angell had stayed only a year and here was Titchener trying to leave after a single year.

short, from 1893 to 1900 inclusive.[15] He began to put the master into English. With Creighton he translated Wundt's *Human and Animal Psychology* in 1894, and revisions of the succeeding editions in 1896 and 1901. With Gulliver he translated the first volume of Wundt's *Ethics* in 1897, and the revisions in 1902. He had actually translated the entire third edition of the *Physiologische Psychologie* at Oxford, and, after revising it at Leipzig, took it to Wundt, only to find that its publication was prevented by the fact that the fourth edition was then ready to appear. In Ithaca he translated the fourth edition, and, holding it for last revisions, found it in turn displaced by the publication of the much enlarged fifth edition. Not discouraged he began again on the fifth and published immediately (1902) the first six of its twenty-two chapters, when his work on the *Manuals* took him away. He translated Külpe's *Grundriss* for publication in 1895, and with Pillsbury he translated Külpe's *Introduction to Philosophy* for publication in 1897. In addition to all this he wrote his own *Outline of Psychology* in 1896, his *Primer of Psychology* in 1898, and began the *Experimental Psychology*, of which the first two volumes appeared in 1901, and which we shall return to presently. It was a remarkable accomplishment, even as great men go.

Meanwhile he was 'settling in.' He was made a full professor in 1895, when still only twenty-eight years old. Graduate students were beginning to work in the laboratory, which was moved to larger quarters in Morrill Hall in 1896.[16] Miss Washburn was his first Ph.D. in 1894. Pillsbury took his degree in 1896, Miss Gamble and Bentley theirs in 1898, Whipple his in 1900.[17] Titchener spent much time on his lectures and insisted, even against the President, upon not lecturing more than once a day. He took over the introductory lectures from President Schurman in 1895 and succeeded with them at once.[18] They remained extremely popular until he relinquished them for a time in 1909.[19]

[15] A complete bibliography of Titchener's writings up to 1917 was published by W. S. Foster, in *Studies in Psychology: Titchener Commemorative Volume*, 1917, 323-337 (publ. in Worcester, but now available at the Cornell Psychological Laboratory).

[16] There had been six rooms for the Laboratory in White Hall; there were at first nine in Morrill, all on the fourth (top) floor. During the next decade the number was increased to eleven. In 1907 the third floor was added, giving 26 rooms in all, and the large psychological lecture-room and demonstrational laboratory were built in the new Goldwin Smith Hall. In 1909 two more rooms were added in Morrill, and the space has since remained unchanged. The elementary lectures were first given in the large lecture-room in the basement of the Library; then they were moved to the Laboratory in Morrill Hall; in 1907 they went to Goldwin Smith Hall. *Vide infra*, note 19.

[17] And others; see the list at the end of this article.

[18] As if he had not enough else to do, he had himself appointed from 1896-98 as "professor in charge of music," and thus really began the Department of Music, which was formally recognized after he had accomplished the preliminary work. Throughout his life he was devoted to music, and he was always proud of this little known activity.

[19] I took the course in 1905 in Morrill Hall with people spilling over into all the adjacent rooms. I can not exaggerate the magnetism of the lecturer. I used to retail the lectures to my room-mates, and the interest must have carried over for they always wanted to be told. I remember the lecture on tonal beats as especially exciting; imagine making beats exciting! I was then an engineering student, and yet it was the memory of these lectures that caused me to change to psychology five years later.

It was during this period that Titchener began to draw away from the philosophers. He was a member of the Sage School of Philosophy, but science, he thought, had nothing to do with philosophy—or at least with philosophers, for he always made the distinction. In all it was a struggle of fifteen years or more, but Titchener finally won his fight for a separation and a division of the spoils of scholarships and fellowships.

With all his working and fighting, the Titchener of the nineties found time for society. He was a member of a congenial group, and I am told he was the 'lion' of many social occasions. He was then, as always, an unusual mixture of decorum and of informal, intimate, sympathetic friendliness.[20] He always felt respect for men much older than himself; and he always expected, and even exacted, it from younger men, no matter what the degree of intimacy. Although he was outspoken in criticism of his friends, there was nevertheless underneath a persistent strain of loyalty, and his antagonism to those who had once transgressed his code was lasting.[21]

The total picture in America is not so easy to draw, nor is it so important. A foreigner settling in a new country is not always welcomed, particularly when necessities of life like laboratories are scarce. Especially may this be true of a dominating personality. Yet Titchener seemed always ready to respond to friendship. Angell had moved across the continent, but Sanford, he felt, welcomed him, and until Sanford's death there remained a warm cordiality between these two men. With Sanford he joined Hall on the staff of *The American Journal of Psychology* in 1895. In general, however, Titchener felt repulsed and isolated, and thus he let his colleagues alone, except in controversy. He was probably not constituted for team-work.

[20]Two anecdotes of the nineties illustrate his rigid sense of decorum. Except for his being willing actually to carry the book-case, they might have happened at any time in his life.

In the early years, when Titchener lived with his family in a sort of dormitory building, he had an extra suite for a study and he let Pillsbury use the second room of it as his quarters. One day Pillsbury was about to carry a small book-case up to the Laboratory. Titchener objected and offered to carry it himself, on the ground that people would think Pillsbury was 'poor,' whereas his own status was beyond question. (Of course, Pillsbury carried the case.)

In those same years the President invited Titchener to dinner, but Titchener refused because the President had not called. The President argued (these matters of decorum have always led to argument with Titchener) that he did not have time to call on all the new men. Titchener then said that the President might at least send his coachman with his card. The President did and Titchener went to dinner!

[21]In these days he was beginning too the collection which later filled his house. He and Mrs. Titchener early agreed that the house should be a museum. In it was a place for anything that represented the intimate habits of other times or other peoples; a wool-wheel and a flax-wheel, a shepherd's crook, devil's masks and idols, pewter galore and old American hardware, hair wreaths, chest handles, the ancestral furniture and silver—I mention only what occurs to me. These articles stand for his anthropological interest in the broad; he was also especially interested in anthropology in the ordinary narrower use of the term. Moreover, the house was a vivarium as well as a museum; the interests of the days with Burdon-Sanderson showed themselves. There were cages of snakes, lizards, a legless lizard, chameleons, horned toads, Gila monsters, finches, parrots, and cockatoels. The laboratory men helped to build the pond for the gold-fish. Then also for Mrs. Titchener he began and continued throughout his life a collection of precious and semi-precious stones, a collection necessarily limited, however, by a professor's income.

His relation to the American Psychological Association was typical. The twenty-six charter-members elected Titchener and Münsterberg at the meeting for organization at Clark University in the summer of 1892. Titchener was present at the first regular meeting the next winter, but he shortly resigned over a difference of opinion that had arisen about some action which he thought a matter of honor. He was host at the Ithaca meeting in 1897, but not a member. He joined again in 1910, but I do not think he ever went to a meeting after 1897. When the Association again met in Ithaca in 1925, he was still a member but did not attend, although he held court for a continuous stream of visitors at his house. The next year he allowed his membership to lapse.

Thus the lines between America, on the one hand, and Titchener, on the other, became sharply drawn. Clark University, because of Sanford and *The American Journal of Psychology*, remained within his world and was later definitely to affect it. The *Psychological Review* was still in the camp of the enemy; naturally Titchener stuck to *The American Journal of Psychology* as the mouthpiece for Cornell, following here a well established German tradition. Until 1917 he never published in any of the Psychological Review Publications except once briefly (1902) and thrice again briefly in controversy (1910, 1912). The record shows clearly that he avoided these journals and found less obvious places when *The American Journal of Psychology* seemed unavailable.

The controversy with Baldwin over reaction-times in the middle nineties was partly an ordinary scientific controversy but also a symptom of something beneath the surface. It is interesting to see now that it hinged on a fundamental national difference that was to become more obvious as American psychology developed. Titchener was saying: we must study the generalized human mind, even if it can be done only with elaborately trained observers; this was the German view. Baldwin was saying: we want to know about all sorts of minds, even if some of them are untrained in laboratory work. It is a similar opposition that developed later between the 'pure' scientific psychology of the generalized, normal, adult, human mind (Titchener) and functional psychology, the psychology of individual differences, mental tests, applied psychology, and lastly behaviorism.[22]

Thus it was in the late nineties that Titchener used James' phrase "structural psychology,"[23] in opposition to functional

[22] These psychologies may seem to the reader unrelated. I think, however, that there is the same dominant motive behind them all, a motive that means that we can correctly speak of an American psychology (with Titchener left out).

[23] *Philos. Rev.*, 7, 1898, 449-465; 8, 1899, 290-299. But cf. K. M. Dallenbach, this JOURNAL, 26, 1915, 473-484, esp. 483.

psychology. The latter had come to be applied to the Chicago product (John Dewey, A. W. Moore, and James R. Angell), although philosophically the American tradition, including Columbia and William James, is functional. In the ebb and flow of discussion and controversy the Cornell school thus came for a decade and even longer to be called 'structural.' Titchener himself, however, always disliked labels as pinning one down and impeding progress. He soon abandoned the word, and more recently 'introspectional' has been substituted for it without the least acquiescence from Titchener. The younger men, who in general like sharp dichotomies, might think of themselves as 'structuralists' or 'introspectionalists,' but not so Titchener. I never heard him refer to his school by any other word than "we."

We are now, however, passing into the first decade of the new century. Here Titchener's publication fell off for he was engaged in a great undertaking, the preparation of the four volumes of the *Experimental Psychology*, popularly known as 'Titchener's Laboratory Manuals.' Some one once said that this is the most erudite psychological work in the English language, and I am inclined to agree, not even excepting James (whose *Principles* Titchener called 'theory of knowledge' and not psychology!). What Titchener wanted to do was to establish psychology as a science by the exhibition of its scientific nature and by the introduction of laboratory 'drill courses' (as he always called them) into university instruction. Sanford's *Course* (1894-98) was not enough; the project needed to be prosecuted upon a larger scale. Titchener worked out all these experiments in the laboratory first. It was not sufficient for him to have the reports of research. He had to devise clean-cut procedures which would require but simple apparatus (for laboratory appropriations were small) and then to see whether students could get univocal results with them. If they did not, instructions could be changed, cautions could be added, procedure could be modified. A tremendous amount of careful, laborious work in the laboratory was necessary. The relation of the volumes to the psychological literature is more obvious. In this respect they are encyclopedic, and also astonishingly accurate. While there must be errors in typography and references, I have still to find one. When the *Quantitative Manuals* were almost completed, G. E. Müller's *Psychophysische Methodik* appeared in 1904, and Titchener had to revise all the way through. However, he never shirked responsibility of this sort and these *Manuals* were published the next year. The *Qualitative Manuals* had appeared in 1901.

It was about this time that Titchener's separatism showed itself in another fashion. Titchener was spiritually, as well as

actually, outside of the American Psychological Association. It did not always follow in "the straight scientific path,"[24] which was of course experimental psychology in the narrower sense in which that phrase is still used. He wanted a group where there could be intimate discussion and constructive criticism of laboratory research then in progress. Later, perhaps even then, he dwelt upon the importance of the attendance at such meetings by graduate students, who thus would hear their seniors in friendly debate. So in 1904 he organized at Cornell a group which has ordinarily been called "the Experimentalists." Nothing could have been more informal in organization. Certain laboratories were to constitute the group, and Titchener always, even up to the time of his death, thought of a laboratory in Wundtian fashion as a man with his staff, his students, and his problems as the common property of all. The 'Experimentalists' have met every spring at different laboratories twenty-four times, omitting only the year of the War. They never had a president, a secretary, a record, a program, or, with a few exceptions, even a printed notice in the journals. There was only, every year, a "host," with Titchener of course as *deus ex machina*. The principle was ideal, especially in over-organized America. Many of us who have attended have felt that these meetings were the most stimulating of the year. As time went on, with Titchener's vigorous personality creating enemies as well as friends, the group became more limited. The bonds were between men and not laboratories; and only those who knew Titchener can realize how completely these meetings centered about him, and why they thus belong, not in a history of their own, but in his biography.[25]

With the *Manuals* out of the way, Titchener's interest in systematization came to the fore. It had never lapsed since the Leipzig days, but in so far as it found its way into print it was more often for the sake of removing an objection than from a pure interest in construction. Mach and Avenarius were necessary to prove psychology a science. 'Structural psychology' was intended as an antidote to 'functional psychology.' The doctrine of the sense-distance in the *Quantitative Manuals* was Delboeuf's and was meant by Titchener to overcome the 'quantity objection' to mental measurement. Now, however, Titchener, almost forty, was ready to cope with the problems of at-

[24] As James said, referring to Titchener.

[25] That the hitherto unprinted record may be complete I give the list of meetings from the first as Titchener gave it at Cornell in 1923.

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|---|---|---|---|
| 1904 Cornell | 1910 Hopkins | 1916 Princeton | 1922 Wesleyan |
| 1905 Clark | 1911 Cornell | 1917 Harvard | 1923 Cornell |
| 1906 Yale | 1912 Clark | 1918 [Omitted] | 1924 Clark |
| 1907 Pennsylvania | 1913 Wesleyan | 1919 Cornell | 1925 Princeton |
| 1908 Harvard | 1914 Columbia | 1920 Cornell | 1926 Pennsylvania |
| 1909 Princeton | 1915 Yale | 1921 Clark | 1927 Harvard |

Ten laboratories were invited to the first meeting.

tention, feeling and thought. There were three years (1905-07) of preparation with little productivity. Then in February, 1908, Columbia University asked him to deliver a series of lectures, and he gave the *Lectures on the Elementary Psychology of Feeling and Attention*, and published them in the same year. It was feeling and attention that he held to be "elementary;" the lectures are not. Attention was then (as now, alas!) a difficult concept. It seemed to imply activity, and 'scientific introspection' shows only content in the mind. How was science to deal with it? Titchener sought to settle the problem of attention by declaring its status as an attribute of sensation. Feeling too was an uncomfortable item. Wundt's theory was not believed, and feeling was always becoming either sensation or, like attention, activity. Titchener sought to give it, as far as possible, independent elementary status, thus preserving for an elementaristic system an additional term with which to work.

The next year he was invited for a lecture series at the University of Illinois. Külpe had slipped from the path that led straight from Wundt, and Külpe's school of 'imageless thought' at Würzburg, while it brought thought into the laboratory as Wundt would not attempt to do, introduced into psychology a fourth element (sensation, image, feeling, thought) which Titchener could not, in the experiments of his graduate students, find. At Illinois Titchener gave the *Lectures on the Experimental Psychology of the Thought-Processes*, which were published the same year (1909). Here he analyzed and combated Külpe's school, seeking to reduce thought to the sensory and imaginal conscious attitudes. Perhaps the most important thing in this book is the context theory of meaning, which psychologizes the supposedly imageless thoughts in a way that explains why they seem to be imageless.

All this while the graduate students in the laboratory were very active, mostly upon problems related to Titchener's then primary interest. Titchener himself was also preparing the *Text-book of Psychology*, intended for instruction in elementary courses, but really a systematic work in brief compass. The first half of it appeared in 1909 and the whole in 1910. It is the only thorough account of Titchener's psychology that we have in single covers. It was printed seventeen years before his death and to the psychological world at large still represents Titchener, for there has come from him nothing else comparable.[26]

Titchener kept on thinking, reading, and working. The system of 1910 was old in 1915, and it has, perhaps several

[26]The *Text-book* is still used in classes, just as James' *Principles* stood for James the psychologist and was used in classes at the time of his death, eighteen years after its publication.

times, been again outgrown. The language of his laboratory changed, and a graduate three years absent had on returning to learn psychology again—not entirely, but some fundamental thing that shot through the entire structure and bobbed up at every point of a discussion. Naturally the Cornell group felt that it was far in advance of psychology in general and of the Titchener who was known outside. There is, however, a striking change in the objective record. The change is not so much in the number of titles in Titchener's bibliography as that there are no books except the *Beginner's Psychology* in 1915,[27] begun as a revision of the old *Primer*, but characteristically written entirely anew. On the surface then there is a difference between the first and the last halves of the thirty-five years in America. Can it be explained?

First, we must note that in 1909 Clark University, then a virgin university for research and graduate instruction only, called Titchener on Sanford's elevation to the presidency of Clark College. Cornell responded by appointing Titchener to a professorship in the Graduate School, thus relieving him of all undergraduate instruction. The laboratory was divided. In 1912, however, when Bentley went to Illinois, Titchener resumed command of both halves of the laboratory (so closely integrated that it had never been really divided), and he also returned to the elementary lectures by way of a special additional appointment.

The result of these changes was the creation of a life of academic retirement. Let me picture Titchener from 1913 to 1917 when I was his "underling."

In the first semester on Tuesday and Thursday at eleven he lectured to the undergraduates in the new lecture-room in Goldwin Smith Hall, the room with a psychological demonstrational laboratory and an office built off it, and with the pitch of the seats determined by Titchener's stature. The demonstration was set out the hour before, and Titchener arrived shortly after ten to inspect it. Later the staff gradually gathered in his office. When the time of the lecture arrived, he donned his gown, the assistant brushed his coat for fear of ashes from the ever-present cigar, the staff went out the door for apparatus and took front seats, and Titchener then appeared on the platform from the office-door. The whole rite was performed pleasantly and sometimes jokingly; yet it was scrupulously observed. After the lecture the staff gathered in Titchener's office for an hour for talk and at one o'clock dispersed for lunch. The Laboratory was across the campus in Morrill Hall and Titchener never went near it during the first semester. That he was able from his house nevertheless to determine some of the smallest details of its operation is a tribute to a personality that could impress its will, generally without resentment, upon others.

In the second semester the lectures ceased and the seminary began on Monday evenings in the Laboratory. Invitation to attend was an honor.

[27] As a matter of record I remark that the original MS., with its corrections, is in my possession. Titchener composed rapidly on the typewriter and revised with pen. In drastic cases of course he rewrote and in all revision he was indefatigable.

The staff were always there as a a matter of course. The seminary brought Titchener to the Laboratory and there was the chance for the lucky graduate to persuade Titchener to look at his apparatus, but he did not always succeed. The seminary ran on, with prandial entertainment provided in rotation by its members, until late—often until after midnight when gas light had to be substituted for electricity.

In June Titchener came to the Laboratory for doctors' examinations, but I do not think that in seven years (perhaps seventeen) he was once in the Laboratory between June and February. Graduates saw him at the house and not often. On my dissertation I conferred with Titchener twice, once when the problem was planned, once eighteen months later when he had gone over the finished manuscript. He had a special assistant and no one else ever saw him in the mornings (except at the lectures in the first semester). Telephoning, for others than the assistant, was taboo except in great emergencies.

In those days Titchener played tennis daily in the summer. During the rest of the year he would on occasion go down town to shop, but otherwise lived almost entirely in the house. He had, I think, absolutely no social engagements except with the laboratory group and with the little orchestra of his family and students that he conducted at home on Sunday evenings. His old-time friends had been much older than himself and were dead or removed from Ithaca. Professors who had long been at Cornell had never seen Titchener. They could not even get into his lectures, for the room was full of sophomores and no one else was admitted.

In the last decade there were changes in this routine, but they do not greatly alter the picture. In 1918 Titchener began to lecture in the summer school for financial reasons. His tennis dropped off at the same time. Some (but not all) graduate students with doctoral research in hand, went to the house weekly for the most careful oversight and instruction.

In 1911 Titchener gave the Lowell lectures (never published), but thereafter he travelled only with great reluctance. He went to the 'Experimentalists' in the spring. At the time of the War he broke his rule to lecture, so that he might contribute to the Prince of Wales Fund for war widows and orphans. He lectured at Wesleyan in 1922 and at the opening of the new Princeton Laboratory in 1925, both in connection with attendance upon the meetings of the Experimentalists. He had planned this present fall to participate in the opening of the new laboratory at Wittenberg College, for he still felt that the founding of a psychological laboratory was the most important physical thing that could happen in psychology.

Now this life of retirement ought to have led to increased productivity. It did not on the record. Is the record at fault?

Perhaps it is in the beginning of the period. From about 1912 to 1917 Titchener was energetically engaged in writing a systematic psychology. His friends supposed then that it was to be a work of several volumes like Wundt's *Grundzüge*. In 1917 the first volume was done all but the last chapter. Had the first volume of a great work appeared in 1918, I suppose we should say that the preceding period was merely one of incubation; but it did not appear. In 1921-22, having become sole editor of *The American Journal of Psychology*, he printed therein two

of the chapters.[28] About this time he seems to have decided not to attempt to continue the work beyond this volume of systematic prolegomena, and also to rewrite the volume in part without again printing these chapters. The book was advertised by the publishers. Titchener's friends urged him to complete it, but there was always some excuse. When an anonymous benefactor offered, in behalf of the book, to relieve him of teaching in the summer of 1927, he hesitated, agreed, but deferred acceptance until 1928.[29] He died without publishing.[30]

I can not presume to say anything of the growth of Titchener's psychology since 1917, but must leave that to some intimate colleague of these years. Already in 1917 there had been a shift in the doctrine of sensation and the attribute.[31] The doctrine of dimensions came later, as did also the stress on phenomenological observation as distinguished from introspection. There have been hints of the 'newest' psychology in theses, but none that are clear to the uninitiated. Only his manuscript or some laboratory intimate of the last few years, who has lived in the community built around the 'system,' can tell us.

Our question, however, is still unanswered. Was there a change about 1910? In objective productivity: yes. In the relation of this magnetic, dominating personality to others: no. In the vigor or quality of psychological thought: we may never know.

My own theory is just a guess and I give it only as such. It is probable that no one works without extraneous incentives—ambitions or irritants. Titchener's graduate appointment in 1910 had removed many irritants. Ambitions were being removed by circumstances. It was becoming plain that Titchener would find no one who would carry on his torch as he had been carrying Wundt's, and he always wanted an able successor-disciple. He thought of Münsterburg's post at Harvard as the greatest position in America; but, when it was offered him in 1917, he found that his picture of a laboratory could not be created at Harvard and he stayed at Cornell with no other place where he could possibly want to go. Except, of course, Oxford; but Oxford seemed no more ready to recognize psychology then than it was a quarter of a century earlier. There was a *Festschrift* for him after twenty-five years of service at Cornell,[32] and it is true that Titchener felt this an honor. He was, however, an alien and ineligible to the National Academy of Sciences. He was a psychologist in the "colonies" and never got his F. R. S. In 1926 he was elected a foreign member of the

[28] Brentano and Wundt: Empirical and experimental psychology, this JOURNAL, 32, 1921, 108-120; Functional psychology and the psychology of act, *ibid.*, 32, 1921, 519-542; 33, 1922, 43-83. Titchener's thoroughness is indicated by the fact that he said that he spent an entire year on Husserl only to find out that "there is nothing in him." The result of this year is four printed pages; *ibid.*, 33, 1922, 55-58.

[29] The growth of the unsuspected cerebral tumour may explain the 'resistance' of the last few years, but the indication is that it was not of earlier origin.

[30] Many of us hope that the publication of what is finished can follow now.

[31] Sensation and system, this JOURNAL, 36, 1925, 258-267. Titchener denied that this article meant a change in his doctrine of attributes, and said that he had held the same view all along. The article certainly was a change from what the men of his Laboratory at that time thought had been his doctrine.

[32] *Studies in Psychology*, *op. cit.*

Polish Academy of Science and Letters, but by this time he had gotten beyond response to such events.[33] He had, by shutting himself off more and more, outgrown even the possibility of external stimulation. I can not assert that an external drive is necessary in a man like Titchener, but I think it is. The fact that the psychological urge in him, as compared with his first two decades in America, had continuously weakened in the last fifteen years is unescapable, compelling as it has always been. He let himself be drawn into other things, like numismatics. He was too young for this change to have resulted from age; one has to look for some other cause.

Titchener's interest in numismatics is significant as showing both his versatiltiy and his thoroughness. He had always, so he once wrote, "been interested, even from fairly early childhood, in Greek and Roman numismatics." In recent years he undertook to learn what he could of Mohammedan coins. He began by "buying up fag ends of lots offered for sale in New York and London," and amused himself by working out the inscriptions. He was very fond of puzzles and here were puzzles where an historical and linguistic knowledge could be acquired and brought to bear. He was borrowing books in 1922 from the American Numismatic Society, and thus came into contact with Mr. Howland Wood, its curator. A close 'numismatic friendship' developed, with a few visits between the two men and a voluminous correspondence.[34] Mohammedan coins were at the focus of this interest, and Titchener learned some Arabic in order to read them.[35] Later his attention was extended to include Chinese coins, but he never lost his original love for the Greek and Roman. Wood would send him coin-puzzles to decipher, or together they would work them out. In all of this work Titchener was the student, but an ideal student who admitted help only in the last emergency. Mr. Wood writes: "He has probably read nearly every book in our library on oriental coins, so thorough was his enthusiasm in pursuing to a finish whatever he undertook to do. . . . I have been increasingly surprised at his mastery in reading the inscriptions on coins after he really got started. An unread coin was a delightful problem to be mastered. . . . His knowledge, versatilty and memory filled me with wonder. His reasoning and deductive powers, given time, would have put him in the first ranks as a numismatist, especially as he was not afraid to ask questions and gratefully accept answers." All this in a field outside of psychology! It is true that it may in part account for the fact that the systematic psychology was never finished, but it shows clearly that there had been no diminution either in Titchener's intellectual vigor or in his capacity for friendships.[36]

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[33]One trouble was that he achieved distinction too soon. His honorary degrees were: D.Sc., Oxford, 1906; LL.D., Wisconsin, 1904; D.Litt., Clark, 1909; D.Sc., Harvard, 1909. After 1910 there would be nothing left to do with degrees except increase the quantity.

[34]Mr. Wood has very kindly shown me some of this correspondence.

[35]I have said nothing of Titchener's command of languages. He retained his training in Greek and Latin from the Oxford days, when he studied classics; he used to make up Latin verses for fun. He was most fluent in German, which he read incessantly and could speak well. He also read French constantly, and spoke it well in his younger days. He could read Italian, but used an Italian-German dictionary when he wished to be accurate. He was naturally a linguist. Burdon-Sanderson at Oxford once gave him an article in Dutch for report a week later. When Titchener said he did not know Dutch, Burdon-Sanderson said, "Learn it." He 'learned' it enough to report in a week. He knew some Sanskrit on account of his interest in philology. The numismatics took him into Arabic and even a little Chinese. One ought to add English to the list, for he was much more than ordinarily fluent in the mother-tongue.

[36]I have not touched as fully as I should like upon the personal side of the older Titchener. He had enemies in youth, derogators in maturity, and he was never old; but, in spite of his separatism, he had always a small group of psychological friends with whom he kept up a voluminous correspondence. Besides these men there was also a much larger group of young men who depended upon him professionally in the criticism of papers, for academic advice, and even in personal affairs. In many there was the mixture of fear and admiration that I have already mentioned. His total correspondence was very large, and he never before 1921 had stenographic assistance.

The best key to Titchener's life, I think, lies in the fact that he emulated Wundt—how consciously I do not know. We have already seen the superficial evidence of the Leipzig impress, but I think the influence goes deeper. Often when Titchener wrote about Wundt, he seemed to be writing about himself. Often when he defended Wundt in print, he used phrases which he has used in defense of himself.[37]

Titchener urged that there were three interests and corresponding periods in Wundt's life, concerned successively with the establishment of experimental psychology, with the creation of a scientific metaphysics, and with the development of *Völkerpsychologie;* and he was most emphatic in his insistence that those interests constitute, not three programs of work, but a single, closely integrated program.[38] The parallel in Titchener holds for the first two periods. He began with the further establishment of experimental psychology as a science (1892-1905); he went on to systematization, that made many say he was no longer an 'experimentalist' (1905-1927); he had always a keen interest in anthropology,[39] and a few of his friends had wondered if he would not turn to anthropological writing when he retired from his active professorship. Yet these interests were not successive, but continuous; he had never been without them all; the dates mean only the dominance of one in personal publication. Titchener thought of them as an integrated whole and defended the unity of the plan in Wundt. He was at pains to show that Wundt foresaw the entire plan of his life in 1862,[40] and just so might Titchener's tripartite psychology be traced from 1892.

In this article on Wundt, Titchener protested against the belief that "social psychology was Wundt's first and fondest love, and that all his life, up to about 1890, was spent in clearing intruders out of the way, that he might ultimately return to it." "I should not accept this legend," he wrote, "if it came with Wundt's own subscription; I should mistrust an old man's memory."[41] Strong words! Could Titchener have been anticipating the criticism that he was not at heart an experimentalist should he presently turn, in his writings, to anthropology? Or could he have been thinking of the criticism that his interest in system called in question the primary nature of his experimentalism?

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[37]Cf. therefore, Titchener's account of Wundt, immediately after Wundt's death, this JOURNAL, 32, 1921, 161-175, to which reference is made below. The suggestion that a thread of autobiography runs through this article of Titchener's must, of course, be taken with extreme caution.

[38]*Op. cit.*, 163-169.

[39]His home contained many curios.

[40]*Loc. cit.*, all, but esp. 163.

[41]*Op. cit.*, 169.

Then there is the discussion of Wundt's fluidity. James once said of Wundt: "Cut him up like a worm, and each fragment crawls; there is no *noeud vital* in his medulla oblongata, so that you can't kill him all at once."[42] Titchener dissented vigorously from this sort of criticism, which is not unlike complaints that were sometimes made of him. Wundt was, he said, "as honest as the day," but his work was always "at once systematic and provisional" and "he held his theories far more loosely than his readers ordinarily suppose;" "his greatest reverence was for fact."[43] Titchener based his assurance in this matter on Wundt's assertions to him in conversation; what intimate of Titchener's is there who has not heard him use almost identical phrases about himself?[44]

There is also what Titchener said of the necessity that Wundt should not be taken externally and superficially and "that students of Wundt must read his books in series, and can never hope to understand him fully from any single presentation of his thought."[45] How often Titchener used this same argument in reply to criticism!

Most of the other similarities between Titchener and Wundt we have already noted as lying in the more objective record. There remains only his editorial attitude. Titchener wanted a journal of his own, like Wundt's *Philosophische Studien*.[46] He never had it, although he practically secured it in 1921 when he became sole editor of *The American Journal of Psychology*. When he found in 1925 that it was not easily possible in America to do what used to be the rule in Germany, he resigned. However, he discovered that the continued *American Journal* and the *Psychological Review Publications* were to him what the *Archiv* and the *Zeitschrift* were to Wundt in 1902. He thought of a Cornell series, analogous, I suppose, to Wundt's *Psychologische Studien*, started under similar circumstances. Actually he finally arranged to participate in the founding of a new journal —a project still incompletely realized at the time of his death.

The evaluation of Titchener's psychology can be left to posterity, and such was his wish, often expressed. He said of

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[42] *Letters of William James*, 1920, I, 263.

[43] Titchener, *op. cit.*, 169-171. Titchener's note that "there is, however, no smoke without fire" (p. 170) gives the reason why Wundt did not acknowledge change publicly, and would seem similarly applicable to Titchener's own thought.

[44] Titchener has always criticized me for writing with "hardened concepts" and for showing, in controversy, my whole hand. He cited G. E. Müller as the ideal, but of course he sought to follow his own ideal. Especially when taken in relation to what he says of Wundt, I think this fact throws light on Titchener's attitude in controversy.

It was to Müller, rather than Wundt, that Titchener wrote. He once said that he always had Müller in mind as his audience, and he may have been trying to hoist Müller with his own petard as well as to follow Wundt.

[45] *Op. cit.*, 171 f.

[46] Although not in make-up, for he had very definite views about notes, reviews, and the other constituents of a general journal.

Wundt's plan of life: "As to the ultimate significance of that task, it would be the part of wisdom to keep silence; we stand too near to Wundt to see him in a just perspective."[47] Certainly Titchener was a remarkable personality and a great psychologist. That his influence has been so effective in spite of his personal isolation from psychologists and his intellectual isolation from the national trend is, I take it, a test of greatness. A century hence it will be possible to say just where his psychology belongs in the history of the science.

EDWIN G. BORING

## CORNELL DOCTORATES IN PSYCHOLOGY

The psychologists who took the doctorate with Titchener are listed below.[48] As the Graduate School was not organized at Cornell until 1910, and as the records before that date are incomplete, the names of some may have been inadvertently omitted.

| | |
|---|---|
| 1894. | Margaret Floy Washburn |
| 1896. | Alice Julia Hamlin |
| | Walter Bowers Pillsbury |
| 1898. | Isaac Madison Bentley |
| | Eleanor Acheson McCulloch Gamble |
| | Stella Emily Sharp |
| 1900. | William Chandler Bagley |
| | Guy Montrose Whipple |
| 1901. | Carrie Ransom Squire |
| 1902. | John Wallace Baird |
| | Margaret Everitt Schallenberger |
| 1905. | Herman Campbell Stevens |
| 1906. | Samuel Perkins Hayes |
| 1907. | Elsie Murray |
| 1908. | Joseph Herschel Coffin |
| 1909. | Ludwig Reinhold Geissler |
| | Taizo Nakashima |
| | William Henry Pyle |
| 1910. | Helen Maud Clarke |
| | Clarence Errol Ferree |
| | Tamekichi Okabe |
| 1912. | Lucy May Day |
| | Austin Southwick Edwards |
| | Alma deVries Schaub |
| 1913. | Karl M. Dallenbach |
| | William Silliman Foster |
| | Christian Alban Ruckmich |
| 1914. | Edwin Garrigues Boring |
| | Mabel Ensworth Goudge |
| 1915. | Josephine Nash Curtis |
| 1916. | Salem Shihadeh George |
| 1917. | Gilbert Joseph Rich |
| 1918. | Cora Louisa Friedline |
| | Josephine Mixer Gleason |
| | Paul Thomas Young |
| 1919. | Hubert Sheppard |
| 1920. | Homer Guy Bishop |
| | Claire Comstock |
| | Forrest Lee Dimmick |
| | Louis Benjamin Hoisington |
| | Robert Thomas Holland |
| | Cheves West Perky |
| | Alice Helen Sullivan |
| | Michael Jacob Zigler |
| 1921. | Anna Kellman Whitchurch |
| 1922. | Mabel Florence Martin |
| 1923. | Grace Kinckle Adams |
| | Wilbert Andrew Garrison |
| | Albert Clayton Reid |
| 1924. | John Paul Nafe |
| | Elizabeth Fallin Möller |
| | Magnus George Scheck |
| 1925. | Samuel Feldman |
| 1926. | Frederick Lovell Bixby |
| 1927. | Joy Paul Guilford |
| | Junichiro Horiguchi |

[47] *Op. cit.*, 176.
[48] I am indebted to Professor Dallenbach for this list.

# A QUANTITATIVE INVESTIGATION OF THE PURKINJE AFTER-IMAGE[1]

By Deane B. Judd, Cornell University

## Introduction

In 1823 Purkinje described a fleeting after-image following a brief stimulation of the visual organ by a light pulse.[2] Since that time many investigators have studied the after-images following momentary retinal illuminations and have referred with considerable inconsistency to one or another of the after-image phases as the Purkinje phase or after-image.[3] Inasmuch as many major contradictions are common in the results reported by different workers, this inconsistency is not surprising. It was not until the publication of a series of papers by Fröhlich (1921),[4] which followed the pioneering work of Dittler and Eisenmeier (1909),[5] that the after-image reported by Purkinje was specifically identified.[6]

Although many investigations have been made on the visual after-images since Purkinje's time, the work of Dittler and Eisenmeier is referred to as pioneering because it was the first to identify and to describe the complete series of after-images that are produced by a momentary retinal illumination. Their descriptions were verified by Fröhlich in such an exhaustive series of experiments that little doubt now exists regarding the accuracy of their findings for the normal observer. The series of phases in the image, according to Dittler and Eisenmeier, is as follows:

(*1*) The primary image (not an after-image).

(*2*) A very short, dark interval (negative phase).

(*3*) A positive after-image (bright),[7] of the same hue as the primary image (Phase *1*) though usually less bright (Hering after-image).[8]

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[1]From the Physical Laboratory of Cornell University.
This is a condensed report of a thesis offered to the Graduate Faculty of Cornell University in partial fulfillment of the requirements for the degree of doctor of philosophy. The complete thesis may be had on application to the Cornell University Library.

[2]J. Purkinje, *Beobachtungen u. Versuche zur Physiologie der Sinne; Beiträge zur Kenntnis des Sehens in Subjective Hinsicht*, I, 1823, 97.

[3]It must be understood at the outset that the relative increase in sensitivity to light of short wave-length which the eye exhibits in twilight vision, and which is sometimes referred to as the Purkinje effect, is connected to the subject of the present investigation only by the fact that the same man first described it.

[4]F. W. Fröhlich, Untersuchungen über periodische Nachbilder, *Zsch. f. Sinnesphysiol.*, 52, 1921, 60-88; Über den Einfluss der Hell und Dunkeladaptation auf den Verlauf der periodischen Nachbilder, *ibid.*, 53, 1922, 79-107; Über die Abhängigkeit der periodischen Nachbilder von der Dauer der Belichtung, *ibid.*, 108-121.

[5]R. Dittler and J. Eisenmeier, Ueber das erste positive Nachbild nach kurzdauernder Reizung des Sehorgans mittelst bewegter Lichtquelle, *Arch. f. d. ges. Physiol.*, 126, 1909, 610.

[6]Carl von Hess, in a long series of publications, has reported results which are so contradictory to the findings of other investigators that they suggest that he has made an error in phase identification. Cf. Fröhlich, *op. cit.*, 53, 1922, 80.

[7]The after-image following a light pulse is called positive if it appears brighter than the surrounding field, and negative if less bright. The use of the terms 'positive' and 'negative' to refer to brightness only, and not to the hue of the image—as is more usual in psychology—is necessitated by the existence of an after-image that is positive in brightness and complementary in hue to the primary image, *e.g.* the Purkinje phase. This terminology was used by H. G. Hamaker in 1899 (Ueber Nachbilder nach momentaner Helligkeit, *Zsch. f. Psychol.*, 21, 1899, 1-44), and has since been accepted by most workers in this field.

[8]This first positive after-image is called the Hering after-image by Fröhlich because Ewald Hering described the method which led Dittler and Eisenmeier to their definite discovery and identification of this phase. Cf. Ewald Hering, Eine Methode zur Beobachtung und Zeitbestimmung des erstes positiven Nachbildes kleiner bewegte Objecte, *Arch. f. d. ges. Physiol.*, 126, 1909, 604.

(*4*) A short, dark interval, darker than the surroundings, a negative phase. It is, however, longer than the second phase (Phase *2*).

(*5*) A second positive after-image, which appears most plainly in the hue complementary to that of the primary image (Purkinje after-image).[9] It is definitely less bright than Phase (*3*).[10]

(*6*) A long, dark interval, a negative image.

(*7*) A third positive after-image of long duration; it is of the same hue as the primary image, but unsaturated. It is less bright than Phase (*5*).

(*8*) The final phase, a long, lingering dark image, negative. This final negative phase does not appear immediately after the disappearance of (*7*) but only after a (usually long) blank interval.[11]

Thus there exist 8 separate phases in the image following a momentary retinal illumination, 7 of which may be called after-image phases. It must not be assumed, however, that these 8 phases can always be observed in every experiment, for such is not the case. The conditions which make it easy to observe the first part of this after-image series, make it difficult to observe the last, and contrariwise.

The experimental conditions upon which the characteristics (such as development time, duration, intensity, and quality) of the after-image series depend, are: (*a*) duration of light stimulus; (*b*) intensity of light stimulus; (*c*) spectral distribution of energy in light stimulus; (*d*) characteristics of the surrounding field; (*e*) characteristics of the field on which the after-image series is projected; (*f*) condition of adaptation of the *O*'s eyes; (*g*) retinal region stimulated; and (*h*) the competency of the *O*s. The complexity of the problem thus becomes evident. The effect of these 8 variable conditions must be determined before the problem is solved.

A great advance was made by Fröhlich when he specified these conditions and worked out the effects of varying (*a*), (*b*), (*f*), and (*g*). He also varied (*c*) — the spectral distribution of energy in the light stimulus — but because these variations were not quantified his results upon this point have only a qualitative significance.

## Problem

Because of the extreme complexity of the after-image series and of the conditions which affect it, we limited ourselves in the present study to the description of the Purkinje phase (Phase *5*) and to the investigation of the conditions of its arousal.

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[9]Although the hue of this phase is complementary to the hue of the primary image (when that image possesses a hue), it is still called a positive after-image phase because it is brighter than the surrounding field. (Cf. footnote 7.) Thus, a primary image consisting of a white circle with a black line across it would be followed by a phase (*5*) consisting of a light circle with a black line across it. If, however, the circular field of the primary image were yellowish white with a black line across it, the phase (*5*) would appear bluish white with a black line across it.

[10]Designated as the Purkinje phase because most workers in referring to any flash image as the Purkinje phase have reported characteristics similar to the above. It has also variously been referred to as 'recurrent vision,' the 'recurrent image,' and 'Bidwell's ghost.'

[11]The interval is referred to as 'blank' because in it the after-image is indistinguishable from the dark surrounding field. It must not be confused with the short negative phases (*2*, *4*, and *6*), which although they do not possess hue, are definitely darker than the dark surrounding field.

Of all the phases of the after-image sequence, the Purkinje is the most interesting, not only because less is known about it, but also because of its apparent paradoxical nature—being positive in brightness and negative in hue. Our work, therefore, bears upon the Purkinje phase. The other 7 phases are mentioned incidentally only for purposes of orientation and elaboration.

## Apparatus and Procedure

Before pointing out the means by which the present work might be expected to fill some of the gaps in Fröhlich's results, it may be well to note that, since Purkinje's description more than a century ago, the attack on this problem has always been by one of two general methods. (1) Either a stationary light pulse has been used, after which the images were described as they appeared successively in time; or (2) a moving light spot or slit has been used—the observing eye being held stationary—after which, at a given time, the images appear simultaneously spread out in space. The first method, used by Brücke,[12] Helmholtz,[13] Aubert,[14] Exner,[15] Young,[16] Davis,[17] Charpentier,[18] Hess,[19] Snellen,[20] Bosscha,[21] and Hamaker,[22] is best adapted to the study of the last part of the after-image series. The second method, used by Purkinje,[23] Bidwell,[24] Von Kries,[25] Hess,[26] Hamaker,[27]

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[12]E. Brücke, Versuche über subjective Farben, *Poggend. Ann. d. Physik u. Chem.*, 84, 1851, 418.

[13]H. von Helmholtz, *Physiologischen Optik*, I, 358 ff.; also *Handbuch der Physiologischen Optik*, II, 1911, 204.

[14]H. Aubert, *Handbuch der gesammte Augenheilkundes*, edited by Graefe and Saemisch.

[15]S. Exner, Ueber den Erregungsvorgang im Sehnervenapparate, *Sitz. d. K. Akad. Wiss.*, 65, 1892, 59.

[16]C. A. Young, Note on recurrent vision, *Phil. Mag.*, 43, 1872, 343.

[17]A. S. Davis, On recurrent vision, *Phil. Mag.*, 44, 1872, 526.

[18]A. Charpentier, *Oscillations Retiniennes, C. R.*, 113, 1891, 147.

[19]Carl von Hess, Bemerkungen zu dem Aufsatz von Bosscha: Primäre, sekundäre und tertiäre Nachbilder nach momentanen Lichteindrücken, *Arch. f. Ophth.*, 40, 1894, 259; 44, 1897, 445. Also *Arch. f. d. ges. Physiol.*, 49, 1891, 190; 95, 1903, 1.

[20]H. Snellen, *Notes on Vision and Retinal Perception*, 1896.

[21]H. P. Bosscha, Primäre, sekundäre und tertiäre Netzhautbilder nach momentanen Lichteindrücken, *Arch. f. Ophth.*, 40, 1899, 22.

[22]H. G. Hamaker, Ueber Nachbilder nach momentaner Helligkeit, *Zsch. f. Psychol.*, 21, 1899, 1-44.

[23]J. Purkinje, *op. cit.*

[24]S. Bidwell, On the recurrent images following visual impressions, *Proc. Roy. Soc.*, 56, 132, 1894.

[25]J. von Kries, Ueber die Funktion der Netzhautstäbchen, *Zsch. f. Psychol.*, 9, 1896, 81; Ueber die Wirkung kurzdauernder Lichtreize auf das Sehorgan, *ibid.*, 12, 1896, 81; Ueber die im Netzhautzentrum fehlende Nachbilderscheinung und über die diesen Gegenstand betreffenden Arbeiten von Hess, *Arch. f. Ophth.*, 42, 1896, 90.

[26]Carl von Hess, Untersuchungen über den Erregungsvorgang im Sehorgan bei kurz und langdauernder Reizung, *Arch. f. d. ges. Physiol.*, 101, 1904, 226; Untersuchung zur Lehre von der Wechselwirkung der Sehfeldstellen, *ibid.*, 179, 1920, 50.

[27]H. G. Hamaker, *op. cit.*

Samojloff,[28] McDougall,[29] Dittler and Eisenmeier,[30] Hering,[31] and Fröhlich,[32] is best adapted to the study of the first part of the series, *i.e.* of the rapidly appearing and disappearing phases (Phases *1-5*).

*Apparatus.* The apparatus used in the present experiment was constructed according to the principles of the first method so as to give control of a stationary light pulse. As this apparatus is described in detail in the January number of this JOURNAL,[33] a brief statement of its several advantages will here suffice. The experimental set-up enabled us: (1) To obtain readily, in recognized units of retinal illumination (photons),[34] a measure of the brightness of the stimulus. Fröhlich is the only experimenter who measured the brightness of his light. He, however, used arbitrary proportional units which have slight significance outside his own work. (2) To vary continuously and through a wide range: (a) the duration of the light pulse (0.005-2.0 sec); (b) the brightness of the light pulse (0.04-20,000 photons); and (c) the brightness of the surrounding field (0.004-20.0 photons). (a) and (b) have been varied experimentally by Fröhlich, but only with the moving-slit method. The brightness of the surrounding field, (c), has almost invariably been held constant at a very low value (darkness). (3) To control and vary the spatial pattern of the stationary stimulus, and thus to study the detail given in every one of the different after-image phases. A study of this kind is practically impossible with the moving-slit method. (4) To use a large number of *Os*.

*Observers.* Most of the experimenters have emphasized the necessity of long practice for observation in this field of research, and they have consequently acted as their own *Os*. Now and again a second *O* has been called in for a casual qualitative report, but the instances are few in number. Observation of the after-image series was so easy with our apparatus that it was found possible to use 8 *Os*, and to obtain quantitative results from all of them. Of the 8 *Os*, 1 is a psychologist, 1 a biologist, 3 are mathematicians, and 3 physicists.

*Procedure.* Two methods of procedure were used in the present study. In the first, Method A, the *Os* were required to describe all the images that they experienced. The stimulus was presented at the option of the *Os* until they were satisfied that they could give an accurate and detailed description of their images. Although the reports of all the after-image phases were recorded, the variation of the stimulus-conditions was governed by the reports of the Purkinje phase.

In the second method, Method B, the brightness of the stimulus was determined which was required just to evoke a sharply defined Purkinje image. The *Os* were required to report 'yes' or 'no' accordingly as they did or did not see an after-image of a line—whose width in the stimulus-field subtended a visual angle of 5′—in the Purkinje phase. The intensity of the light was varied and the liminal value necessary to produce the after-image of the line in the Purkinje phase was determined for every *O*, and this value was taken as the measure of sensitivity for the Purkinje phase under the different stimulus-conditions. The *Os* were instructed to con-

[28] A. Samojloff, *Zur Kenntnis der nachlaufenden Bilder*, *Zsch. f. Psychol.*, 20, 1899, 118.

[29] W. McDougall, The sensations excited by a single momentary stimulation of the eye, *Brit. J. Psychol.*, 1, 1904, 78.

[30] R. Dittler and J. Eisenmeier, *op. cit.*

[31] E. Hering, Eine Methode zur Beobachtung und Zeitbestimmung des erstes positiven Nachbildes kleiner bewegte Objecte, *Arch. f. d. ges. Physiol.*, 126, 1909, 604.

[32] F. W. Fröhlich, *opp. citt.*

[33] D. B. Judd, Apparatus for the control of stationary light stimuli, this JOURNAL, 38, 1927, 107-112.

[34] Diffuse light of one candle per square meter of radiating surface gives one photon of retinal illumination with a pupillary opening of one square millimeter.

centrate their attention upon the disappearance of the detail rather than upon the disappearance of the phase itself, as we found that such judgments were less difficult and less fatiguing.

## Results: Experimental

*I. Identification of the after-image phases.* Using the method of complete description (Method A), the after-image phases forming the series reported by Dittler and Eisenmeier, and corroborated by Fröhlich were first identified.

Table I
Spectral Distribution of Energy for the Light Stimuli

| Wave length in mμ | Transmissions | | | | Emission |
|---|---|---|---|---|---|
| | Red | Orange | Green | Blue | Black body Radiator at 2790° K. |
| 380 | 0.0 | 0.02 | 0.00 | 0.080 | 0.07 |
| 390 | 0.0 | 0.02 | 0.00 | 0.080 | 0.09 |
| 400 | 0.0 | 0.02 | 0.01 | 0.082 | 0.11 |
| 410 | 0.0 | 0.02 | 0.01 | 0.084 | 0.13 |
| 420 | 0.0 | 0.02 | 0.02 | 0.086 | 0.16 |
| 430 | 0.0 | 0.02 | 0.04 | 0.089 | 0.18 |
| 440 | 0.0 | 0.02 | 0.07 | 0.115 | 0.22 |
| 450 | 0.0 | 0.02 | 0.10 | 0.131 | 0.26 |
| 460 | 0.0 | 0.02 | 0.17 | 0.141 | 0.29 |
| 470 | 0.0 | 0.02 | 0.25 | 0.142 | 0.33 |
| 480 | 0.0 | 0.02 | 0.34 | 0.129 | 0.37 |
| 490 | 0.0 | 0.02 | 0.46 | 0.106 | 0.42 |
| 500 | 0.0 | 0.02 | 0.51 | 0.077 | 0.47 |
| 510 | 0.0 | 0.02 | 0.52 | 0.052 | 0.53 |
| 520 | 0.0 | 0.03 | 0.53 | 0.036 | 0.58 |
| 530 | 0.0 | 0.11 | 0.53 | 0.025 | 0.63 |
| 540 | 0.0 | 0.20 | 0.52 | 0.018 | 0.70 |
| 550 | 0.0 | 0.30 | 0.50 | 0.013 | 0.76 |
| 560 | 0.005 | 0.38 | 0.44 | 0.010 | 0.82 |
| 570 | 0.01 | 0.49 | 0.36 | 0.009 | 0.88 |
| 580 | 0.02 | 0.61 | 0.30 | 0.008 | 0.94 |
| 590 | 0.03 | 0.62 | 0.25 | 0.008 | 1.00 |
| 600 | 0.07 | 0.61 | 0.19 | 0.007 | 1.06 |
| 610 | 0.17 | 0.61 | 0.13 | 0.006 | 1.12 |
| 620 | 0.28 | 0.62 | 0.07 | 0.005 | 1.18 |
| 630 | 0.41 | 0.65 | 0.05 | 0.004 | 1.25 |
| 640 | 0.51 | 0.68 | 0.04 | 0.002 | 1.31 |
| 650 | 0.61 | 0.69 | 0.03 | 0.002 | 1.38 |
| 660 | 0.66 | 0.70 | 0.02 | 0.002 | 1.44 |
| 670 | 0.68 | 0.70 | 0.01 | 0.001 | 1.50 |
| 680 | 0.69 | 0.71 | 0.01 | 0.001 | 1.56 |
| 690 | 0.70 | 0.71 | 0.00 | 0.000 | 1.63 |
| 700 | 0.70 | 0.72 | 0.00 | 0.000 | 1.68 |
| 710 | 0.71 | 0.72 | 0.0 | 0.0 | 1.73 |
| 720 | 0.71 | 0.72 | 0.0 | 0.0 | 1.79 |

Since the most characteristic attribute of the three positive after-image phases is their hue, whether it be the same as the primary image or complementary, a stimulus yielding a colored primary image was employed. This stimulus consisted of the image of a lantern slide over whose four quadrants four different gelatine filters were placed, which will be hereafter designated as Red, Orange, Green, Blue. The transmissions of these four filters found by the spectrophotometer are given in Table I.

TABLE II
Color Characteristics of Stimuli

| Identification of Light Beam | Dominant Wave length mμ | Purity | Transmission for 2790°K. |
|---|---|---|---|
| Black-body radiator at 2790° K. | 586 | 0.61 | 1.000 |
| Bb beam through Red filter | 627 | 1.00 | 0.090 |
| Bb beam through Orange filter | 593 | 0.93 | 0.425 |
| Bb beam through Green filter | 559 | 0.79 | 0.326 |
| Bb beam through Blue filter | 480 | 0.39 | 0.016 |

The color temperature of the source lamp was determined by color matching with a Bureau of Standards lamp whose color temperature had been previously determined as a function of voltage at the Bureau of Standards, Washington, D. C. At a voltmeter reading of 110 (the voltage at which the source lamp was maintained throughout the bulk of the observations) this color temperature was found to be 2785 ± 5° K. This determination was further checked by measuring in watts per mean horizontal candle the power consumed by the source lamp at the same voltage. The color temperature corresponding to this efficiency (0.859 watts per mean horizontal candle) was obtained by interpolating between values previously determined by Forsythe.[35] The color temperature so yielded was 2800° K. with an uncertainty of about 12° arising not so much from experimental error in measuring the light emission as from possible errors in passing from the mean horizontal candle power, which was measured directly, to spherical candle power, the units used by Forsythe. The value of the color temperature finally taken was 2790° K. This accuracy in source specification was necessary because (as will appear later) a change in color temperature of source of 50°, although scarcely perceptible as a color difference in the primary image, produces a change in sensitivity for the Purkinje image that is easily measurable. The emission of a black body radiator at 2790° was found approximately in proportional units from a curve embodying calculations by Priest,[36] and is included in Table I.

The color characteristics produced by a beam from a black-body radiator at 2790° K., together with the color characteristics produced by filtering that beam through the gelatines (Red, Orange, Green and Blue) were computed by a procedure essentially like that recommended by the Committee on Colorimetry for 1920-21.[37] The results of this calculation appear in Table II.

[35] W. E. Forsythe, *Phys. Rev.*, 18, 1921, 147.
[36] L. T. Troland, Report of committee on colorimetry for 1920-21, *J. Opt. Soc. Amer.*, 6, 1922, 561.
[37] L. T. Troland, *op. cit.*, 580-585.

The transmissions were computed from the color excitation sums by means of the chromatic visibility coefficients which refer to the O. S. A. excitations that were used as the basis of the color computations.[38]

When this four-color slide was used the *Os* descriptions of their after-images varied uniformly with a change of the intensity of the light. There was a very close agreement among the reports of the 5 *Os* who served in this part of the experiment. Less than 1% of the readings showed individual differences. The experiments were commenced after 10 min. darkness adaptation. The fixation-point was adjusted so that it was at the exact center of the exposure field. The duration of the stimulus was 0.14 sec.—close to the optimum for the Purkinje phase.

The following phases were reported by all the *Os*. (1) The primary image which corresponds to Dittler and Eisenmeier's phase (*1*). (2) A flash-image which corresponds to their (*3*) or (*5*), *i.e.* to the Hering image or to the Purkinje image, or to both. (3) A diffuse positive image, colored like the primary but of poor saturation which corresponds to their third positive image, to phase (*7*). (4) A final negative phase, light or dark gray and without hue, which corresponds to their final negative phase (*8*).[39]

As the brightness of the stimulus was increased from the value which just elicited the 'flash-image,' so called because of its duration (estimated to be between 1/5–1/2 sec. in length), the following phases, which we shall call 'stages' to distinguish them from the four phases just mentioned, were reported:

(a) The lowest brightness, less than 10 photons,[40] gave a flash-image that was gray for all four quadrants of the slide. This is plainly the Purkinje image. Fröhlich similarly reports that, with increasing intensity of illumination, the Purkinje image is the first fleeting image to appear,[41] and that a colorless Purkinje image can be aroused after 10 min. dark adaptation with a stimulus too weak to elicit a colored flash-image.[42]

(b) When the brightness of the stimulus is increased four times, all the quadrants are seen in the flash-image as unsaturated violet. This stage was not noted by Fröhlich. In point of fact, he denies its existence, for he states that the Purkinje image is always complementary in hue to the primary image. Other workers are not so positive,[43] and there seems moreover

[38] D. B. Judd, Chromatic visibility coefficients by the method of least squares, *J. Opt. Soc. Amer.*, 10, 1925, 646.

[39] The final negative phase never appeared immediately after the third positive image, but always after a noticeable blank interval. This description corresponds to that given by Dittler and Eisenmeier. Our *Os* were many times uncertain whether the final negative image appeared at all; but whenever it did, it was always preceded by this blank interval.

[40] It is impossible to be more specific regarding the stimulus brightness corresponding to this stage because—as will appear later—this value varies a hundred-fold, depending upon the wave length of the stimulus, and upon the individual difference of the *O*.

[41] *Op. cit.*, *Zsch. f. Sinnesphysiol.*, 52, 1921, 73.

[42] *Op. cit.*, *ibid.*, 53, 1922, 90.

[43] Dittler and Eisenmeier (*loc. cit.*) merely say that the Purkinje image appears most plainly in the hue complementary to that of the primary image; and Hamaker (*loc. cit.*) simply says that it is complementary in most cases.

to be ample evidence of the existence of this violet flash-image that is independent of the hue of the primary image.[44]

Two of the 8 *O*s failed to report this stage. As the intensity of the stimulus was increased they passed directly from stage (a) to stage (c). Thus 2 of our *O*s agree with Fröhlich, and 6 definitely contradict him. Since stage (b) is continuous with the image that is complementary to the primary image, and since the majority of our *O*s report it, we think it reasonable to assume that it corresponds to the Purkinje image. That all our *O*s did not report it is due we think to their individual differences. If this be true, the importance of using more than one *O* becomes evident.

(c) When the brightness is again increased fourfold, the flash-image becomes complementary in hue to the primary image. This is without question the Purkinje phase.

(d) When the brightness is again quadrupled, the flash-image is described as the same in hue as the primary image, but tinged with violet of low saturation.[45] This is probably a superposition of the Hering phase (Phase *3*) upon the Purkinje phase (Phase *5*). Fröhlich, in an attempt to account for the contradictory observations reported in the earlier work, showed that the dark phase (Phase *4*), intervening between the Hering and Purkinje phases, would disappear at a proper selection of the stimulus-intensity and thus give the impression that the two phases were one.[46] Fröhlich does not specify the stimulus-brightness that produces this effect so a recheck of his results could not be made. Our *O*s definitely reported this stage as a unitary experience so, if Fröhlich is correct in his explanation, it will appear that the stationary light pulse method used in the present investigation is too ill-suited for phase identification to bring out this fact.

(e) If the brightness of the stimulus is further increased—doubled or tripled—the flash-image is reported as like the primary image in hue. This is plainly the Hering phase. The Purkinje phase fails to appear. The Hering phase becomes more prominent, at the expense of the Purkinje phase, as the brightness of the stimulus is increased.

(f) In some cases, near the upper limit of the scale of stimulus-brightness afforded by the apparatus, the flash-image was described as double in nature; the first part being of the hue

[44]Bidwell (*loc. cit.*) reports that the hue of the 'ghost' is generally violet; Hamaker (*loc. cit.*) reports that the flash-image following a blue primary image is always violet.

[45]One *O* consistently reported a yellow-green or olive flash-image following a blue primary image.

[46]*Op. cit.*, *Zsch. f. Sinnesphysiol.*, 52, 1921, 73.

of the primary image; the second part being complementary.[47] The first part of this double-flash is, we believe, the Hering phase (Phase *3*), and the second part is the Purkinje (Phase *5*).

*II. Spectral effectiveness.* From the *Os*' reports of stages (b), (c), and (d) we determined the intensity of stimulus that best brought out the complementary Purkinje image.

The brightness of the stimulus was first calculated in ml., from the brightness of the unfiltered stimulus beam and the integral transmissions of the filters (see Table II), and then changed to photons (see Table IV) by mulitiplying the ml. by the constant 48, which is the conversion factor that corresponds to a pupil diameter of 4.4 mm. We did not measure the pupil diameters of our *Os* but took the average value of the pupil diameter obtained by Reeves,[48] and by Holladay[49] for adaptation to darkness and fixation 11 in. distant.

Fig. 1 shows that light of short wave lengths is more effective in exciting the complementary Purkinje image than light of long wave lengths.

This fact has been observed by several workers, but Fröhlich is the only one who has given quantitative results.[50] It is impossible, however, to compare his results with ours because he neglected to give the spectral transmission of his filters and the brightness of his light. He used daylight as his source of illumination which is, as is well known, subject to the varying conditions of the weather. Fröhlich, nevertheless, points out that light of the shorter wave lengths excites the Purkinje image much more readily than does that of the longer. His results are, therefore, in essential agreement with those presented in Fig. 1.

The effectiveness of the different wave lengths, as shown in Fig. 1, is the reciprocal of the optimum brightness for the complementary stage (c). If this brightness is exceeded in the stimulus, the brightness of the image may or may not increase—probably would increase, but that point has not been settled by our work. But it is known that when the optimum brightness in the stimulus is exceeded, the complementary character of the flash-image is not so marked; and when it is greatly exceeded, the flash-image becomes almost of the same hue as the primary image. The optimum brightness is not measurable with a high degree of precision, but its reliability is comparable to that of the liminal brightness. The probable error of the points plotted in Fig. 1 (the average of 5 *Os*) is: Red—28%; Orange—17%; Green—200%; Blue—16%. The probable error for Green is high because Green, alone of all the colors used, had a complementary

[47]The *Os* uniformly reported this double image for the Orange and Green quadrants, which were much brighter than the other two quadrants of the slide. Only occasionally did an *O* report this double image for the Red or Blue; and when he did he was always uncertain about it. It is possible that still higher intensities of stimulus would bring out the double images for all the colors.

[48]Prentice Reeves, Rate of pupillary dilation and contraction, *Psychol. Rev.*, 25, 1918, 330.

[49]L. L. Holladay, The fundamentals of glare and visibility, *J. Opt. Soc. Amer.*, 12, 1926 310.

[50]*Op. cit.*, *Zsch. f. Psychol.*, 52, 1921, 74.

which is a fairly close match for the violet-white stage. When *O* reports a violet flash-image, there is no way of telling whether the violet is due to the violet-white stage (b), to the complementary stage (c), or to the beginning of stage (d).

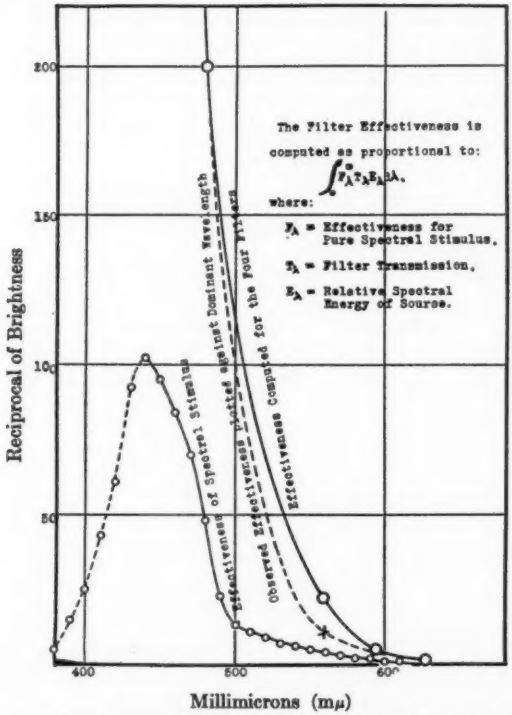


FIG. 1. Showing correlation between wave length and brightness of stimulus that evokes the best complementary Purkinje image.

The effectiveness of the four filters (see dotted curve through X, Fig. 1) is plotted against the dominant wave length. The dominant wave length represents the *color* effect of light of the entire group of wave lengths present in the beam by specifying the wave length of the pure spectral light which, when mixed with white light, produces a color-match. If we wish to compare the filtered beams in brightness, these brightnesses may be set proportional to:

$$\int_0^\infty V_\lambda T_\lambda E_\lambda d\lambda$$

where: $V_\lambda$ = visibility of light of wave length $\lambda$, or the reciprocal of the intensity of light of wave length $\lambda$ required to match a standard brightness.
$T_\lambda$ = transmission of the filter for wave length $\lambda$.
$E_\lambda$ = relative energy emitted by the source of light for wave length $\lambda$.

This integral can be evaluated (in fact, was evaluated in the calculation of the integral transmissions of the filters) because $V_\lambda$ is known.

TABLE III

Effectiveness of Spectral Stimulus to Evoke Complementary Stage

| Wave length in mμ | Effectiveness[51] | Wave length in mμ | Effectiveness |
|---|---|---|---|
| 380 | 5 v | 510 | 11 |
| 390 | 25 v | 520 | 9 |
| 400 | 25 v | 530 | 8 |
| | | 540 | 7 |
| 410 | 43 v | 550 | 6 |
| 420 | 61 v | | |
| 430 | 92 v | 560 | 4 |
| 440 | 102 | 570 | 3 |
| 450 | 95 | 580 | 2 |
| | | 590 | 2 |
| 460 | 84 | 600 | 1 |
| 470 | 70 | | |
| 480 | 48 | 610 | 1 |
| 490 | 23 | 620 | 0 |
| 500 | 13 | 630-720 | 0 |

If we wish to compute the relative effectiveness of the filters in producing the complementary stage of the Purkinje image, this effectiveness may be set proportional to:

$$\int_0^\infty F_\lambda T_\lambda E_\lambda d\lambda$$

where: $F_\lambda$ = effectiveness of spectral stimulus of wave length $\lambda$, or the reciprocal of the intensity of light of wave length $\lambda$ which best evokes the complementary stage of the Purkinje image.
$T_\lambda$ and $E_\lambda$ are defined as above.

[51] Just as $V_\lambda$ may be built up from the color excitations by applying chromatic visibility coefficients (Troland, *op. cit.*, 549, 551; Judd, *op. cit.*): R—0.45; G—0.54; B—0.01; so may F be built up from the color excitations by applying chromatic effectiveness coefficients: R—0.00; G—0.06; B—0.94. From the very high value for the last coefficient (B) may be seen just how close Bidwell's theoretical suggestion (Bidwell, *op. cit.*, 142) that the 'ghost' is due to action of the violet fibers, alone, is borne out quantitatively. As has been mentioned, Bidwell ascribed the 'ghost' to the violet fibers presumably because his experimental conditions led in great part to the production of the whitish-violet stage (stage b). The above confirmation somewhat paradoxically is derived from measurements on the complementary stage (stage c). The suggestion that the Purkinje phenomenon is due to the action of the same retinal mechanism to which is sometimes ascribed the blue, or violet, sensation, merely because the solution for the spectral effectiveness may be thrown into closely analogous form, must not be taken too seriously. The solution given above is not the only one that will satisfy the data which consists of determinations on four points only. Furthermore, the solution was built up by a cut-and-try adjustment of the color excitation sums referring to the filtered light. It is conceivable that the extraordinary resemblance to the blue or violet excitation is due as much to the method of computation as to the observations themselves.

This integral cannot be evaluated because $F_\lambda$ as a function of $\lambda$ has not been evaluated. However, the reverse process is possible. By experiment the effectiveness of the filtered beams has become determined. We, therefore, inquire what values of $F_\lambda$ will yield by computation the same effectiveness. With only four filters whose value for integral effectiveness must be fitted, it is quite likely that a number of solutions are possible for $F_\lambda$. By cut-and-try methods, one of these solutions has been worked out, and is included in Table III.

A plot of the spectral effectiveness is included on Fig. 1. The solid portion of the curve may be regarded as fairly well established by our results. The dotted portion (corresponding to the wave lengths 380-430 m$\mu$, marked 'v' in Table III) is roughly proportional to the visibility. We nevertheless feel that this part of the curve is approximately correct because a stimulus that is not visible is not likely to evoke the comple-

TABLE IV

| Dominant Wave length in m$\mu$ | Optimum Brightness of Stimulus | Observed Effectiveness* | Computed Effectiveness | % Error | P. E. of Observed Effectiveness |
|---|---|---|---|---|---|
| 480 | 16 | 200 | 201 | 0.5 | .16 |
| 559 | 312 | 10.3 | 22.2 | 120 | 2.00 |
| 593 | 780 | 4.0 | 5.2 | 18 | .17 |
| 627 | 1720 | 1.8 | 1.1 | 39 | .28 |

*Unit effectiveness corresponds to 3200 photons. To calculate the observed effectiveness divide 3200 by the optimum brightness in photons.

mentary stage of the Purkinje image. As far as the data taken in the present work are concerned these values are undetermined because of the use of a source lamp of radiating characteristics like a black body at 2790° K. The energy emitted at wave lengths shorter than 430 m$\mu$ is so small a proportion of the total that no information can be yielded about the spectral effectiveness in this region.

As the last two columns of Table IV show, the values of $F_\lambda$ (given in Table III) satisfy the observed data on filter effectiveness to the degree of accuracy indicated by the probable errors. Although it is possible that an extension of the cut-and-try method would yield another solution for $F_\lambda$ which would fit the data more closely, further work was not thought advisable because of the large probable error of the experimental results. It must be emphasized that the readings on which the observed effectivenesses are based were not intended to lead to spectral effectiveness, but were taken only for the purpose of phase identification. The evaluation of spectral effectiveness is therefore incidental in our work; it remains for other inves-

tigators to obtain absolute results regarding the spectral effectiveness for the complementary stage of the Purkinje image.

*III. Distinctness of the images in the different phases.* One of the most striking characteristics of the flash-image is the distinctness of its detail. It is so difficult to measure this by the moving slit method that no one seems to have attempted it; but the stationary light pulse method is admirably adapted for this determination.

Four sets of concentric circles (alternating black and white) were used in this part of the work, one set on each of the four quadrants of a slide. The circumferences of the circles in quadrant 2 were twice the width of those in quadrant 1, in quadrant 3 twice the width of those in quadrant 2, and in quadrant 4 twice the width of those in quadrant 3. The circumferences of the circles in quadrant 1 gave a retinal image of 0.007 mm. in width—about twice the distance that two dots must be separated to be distinguished as two[52]—so we assume that the circles in quadrant 1 were separated about 2 j.n.d.'s. The retinal separation of the circles in quadrant 2 would then correspond to 4 j.n.d's, in quadrant 3 to 8, and in quadrant 4 to 16 j.n.d.'s.

The *O* remained in darkness for 10 min. before the experiments were commenced. He was then instructed to report the number of quadrants on which he distinguished the circles in the primary image, in the flash-image, and in the third positive image. He was allowed to look at the stimulus as often and as long as he liked, until he was satisfied that he could give a correct report.

The fixation point was placed at the center of the stimulus. The retinal image of the stimulus then extended approximately 13° from the foveal pit. The brightness of the stimulus was held constant at 33.5 ml., and the duration was varied among the three intervals, 0.124, 0.031, and 0.008 sec. Five brightnesses of the surrounding-field, including darkness, were used, but the results with this variable were so diverse and so inconclusive that a description of them is not warranted. In general, however, it was found that the use of a moderately bright surrounding-field increased the distinctness of both the primary and the flash-images.

The results show that the flash-image is about ¾ as distinct as the primary image. Four of the 6 *O*s saw just as many of the quadrants covered with circles, on some trials, in the flash-image as in the primary image. One *O*, at 0.124 sec. exposure and with a surrounding-field illumination of 0.03 and 0.10 ml., reported a sharpness of the flash-image which enabled him to discriminate as circles the Purkinje image of quadrant 1. We say, therefore, that this Purkinje image had a distinctness corresponding to 2 j.n.d.'s.

Only a few measurements were obtained for the third positive phase. These, however, indicate that the distinctness of this phase corresponds to 8 j.n.d.'s. This phase is much more diffuse than the flash-image, and in general appeared only as a gray blotch.

No detail was ever reported by the *O*s in the final negative phase.

[52]H. von Helmholtz, *Physiological Optics*, Eng. trans., II, 1924, 37.

Five of the 6 *Os* serving in this part of the experiment reported a very interesting phenomenon which should be mentioned at this point. With a light surrounding-field a negative flash-image appeared between the positive flash-image (Phase *5*) and the third positive image (Phase *7*). This image had a distinctness corresponding to 4 j.n.d.'s. It is termed negative because the black lines of the circles appeared white and the white appeared black. This report was given again and again, and check experiments were made with a slide showing a large black cross on a yellowish white field. As the bluish-white Purkinje image (Phase *5*) of the field faded into the dark phase (Phase *6*), the cross again flashed up momentarily and appeared distinctly brighter than the surrounding field. Then the usual third positive phase (Phase *7*) appeared in which the image was that of a black cross on a light gray field. Though this intermediate phase has never before been reported we did not carry the investigation further.[53]

*IV. Sensitivity, a function of stimulus-duration.* When the brightness of the stimulus is increased above the lower limit of the range adequate to the flash-image, a gray phase (Purkinje Phase *5*) separates from the third positive phase (Phase *7*). It is difficult to determine the exact point at which this occurs for, as Fröhlich points out, the beginning of the third positive phase first becomes a little lighter and then it gradually separates from the dim remainder. This remainder however, is much more diffuse than the Purkinje phase. Hence the *Os* were instructed in the next series of experiments (Method B) to concentrate their attention upon the appearance of a detailed image. The stimulus-brightnesses obtained in this way were approximately twice as high as those obtained by concentrating the attention upon the appearance of a flash-image.

A light surrounding-field (0.005 ml.) was used instead of a dark field because: (1) a condition of adaptive equilibrium can be reached in 10 min. while with the dark field this state is reached only after 1 hr.; and (2) equilibrium condition once reached is stable, while with the dark field the bright stimuli presented to evoke the Purkinje image seriously disturb the adaptation level, at times toward light adaptation and at other times toward dark adaptation.[54] The slide used in the liminal determinations showed a series of fine, black, crosslines (width 0.5 mm.) on a yellowish-white background (radiating characteristics of black body at 2790° K.) on which was also superposed a heavy irregular, black cross (width of line, 7 mm.). The heavy cross was used because its image was always visible in the third positive phase (*7*) while the fine crosslines never were. This arrangement made easy the distinction of the two phases, (*5*) and (*7*).

Fig. 2 (a typical curve of our results) shows the variation of liminal brightness with duration of stimulus. It is high for short durations, falls as the duration is increased, reaches a minimum at about 0.16 sec., and then rises again at the larger durations. Each point plotted is the mean of four liminal set-

[53] In one of Von Hess' early papers (*op. cit.*, *Arch. f. Ophth.*, 40, 1894, 259) a negative image of very short duration was reported, but it was subsequently identified as one of the usual negative phases in which the dark parts of the primary image did not change to bright.

[54] The effect of flashes of light well above the limen, on the course of adaptation to darkness has been studied. Cf. H. v. Helmholtz, *op. cit.*, ii, 1924, 323.

tings, and has a probable error of 25%. The curve, for every *O*, is accurate enough to evaluate consistently the time interval within which a sharply defined Purkinje phase readily appears. The time in sec., corresponding to the two places where the curve crosses the 120 photon mark, has been recorded on Fig. 2 as a measure of this time interval for each of the 6 *O*s. The lowest stimulus-brightness which excites a sharply-defined Purkinje phase is also given in photons for every *O*, as is also the time corresponding to this minimum brightness (the optimum time). [The *O*s agree closely in the values for the optimum time,

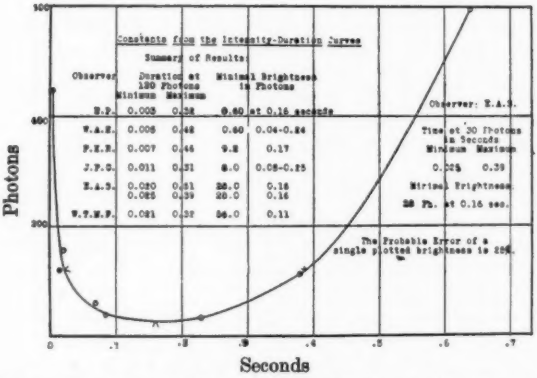
Photons
Seconds
Constants from the Intensity-Duration Curves
Summary of Results
Observer
Duration at 120 Photons Minimum Maximum
Minimal Brightness in Photons
W.P. 0.008 0.38 0.60 at 0.16 seconds
W.A.H. 0.006 0.48 0.60 0.04-0.24
F.K.H. 0.007 0.46 9.8 0.17
J.P.O. 0.011 0.31 8.0 0.08-0.25
E.A.S. 0.020 0.31 26.0 0.18
0.025 0.39 20.0 0.16
W.T.M.P. 0.021 0.32 56.0 0.11
Observer: E.A.S.
Time at 20 Photons in Seconds
Minimum Maximum
0.24 0.39
Minimal Brightness
28 Ph. at 0.16 sec.
The Probable Error of a single plotted brightness is 25%.

FIG. 2. Showing correlation between duration of stimulus and photons of intensity required to evoke just noticeable detail in the Purkinje image.

but differ greatly (100 to 1) in the brightness values required at this duration. It seems reasonable to conclude, however, that the average *O* will see a sharply-defined Purkinje phase if the duration of stimulus is between 0.02–0.30 sec., and the brightness of the stimulus is between 200–800 photons. The conclusion, however, probably holds only for stimuli that possess radiating characteristics close to those of a black body at 2790° K.[55]

The uncertainty of the determinations is too great to warrant the casting of our results in the form of an equation; still, the curves always rose with short durations and indicate that, within the probable error, the product of the duration and the

[55]The color of the stimulus was not varied in this experiment. Color has been found to be of great importance in this work.

stimulus brightness is a constant. This suggests that a retinal element must absorb a certain quantum of light energy before it gives rise to a sharply defined Purkinje phase.

The subsequent rise in liminal brightness for increasing durations of stimulus quite plainly corresponds to factors of a kind entirely different from the cause of this initial decrease. At these durations (0.2–0.5 sec.) the failure of *O* to experience a detailed flash-image was due (1) to the rise of the final negative phase which blotted out the flash-image, and (2) to the fact that the primary image was so dazzling that it showed no detail, in which case the Purkinje phase would, of course, show none.

It must be admitted that the causes of the changes represented by the summary of results in Fig. 2 is somewhat obscure. It is likely that the chief value of this material lies in the fact that it defines the conditions of intensity and duration of stimulus under which the sharply-defined Purkinje phase may be expected to appear.

*V. Sensitivity, a function of dark adaptation.* If an attempt is made to determine with a dark surrounding-field, the liminal brightness just required to evoke a detailed Purkinje image, the results show that during the first 10 min., approximately, the liminal values decline and appear slowly to be approaching a constant. As the work is carried further, and the stimulus intensities reduced still more to determine another limen, the *Os* frequently observe that though the intensity of the stimulus has been decreased the Purkinje image seems much plainer than before. The next limen is always much lower—usually about 1/10 the previous value. It is evident therefore that there is a discontinuity in the rate of adaptation.[56]

The brightness at which this occurs was measured for 4 *Os*.

The same slide was used as in *Part IV*; the duration of stimulus was held constant at 0.11 sec. The surrounding-field was dark; the fixation point was placed at the center of the stimulus and was adjusted before each determination to a brightness that did not distract attention from the Purkinje image. *O*, after looking out of the window for 5 min., commenced liminal determinations (Method B), approaching first from the side of higher stimulus intensities, then, in the next determination, from the side of lower intensities.

During the first 10 min., the Purkinje phase was blue-violet where the stimulus corresponded to the radiating characteristics of a black body at 2790° K. After the sudden increase in sensitivity, this Purkinje phase at close to liminal brightnesses

[56]This sudden change in the rate of adaptation is indicated in the work of Fröhlich (*op. cit.*, 53, 1922, 79-107) though it is not emphasized by him. It is gratifying to report that our work verifies his in this respect.

was described as 'colorless,' 'slate gray,' or 'gray.'[57] The lines which were black in the primary image always appeared black in the Purkinje phase also. It must be emphasized that detail still appeared in the Purkinje image when color of a definite hue had become almost indistinguishable.

The results of a typical experiment of this sort are plotted in Fig. 3. The liminal brightnesses corresponding to a given number of minutes following light adaptation are shown reciprocally after the manner of Fröhlich,[58] and Piper.[59] The

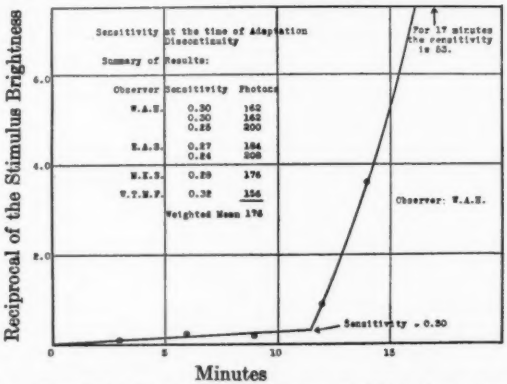


FIG. 3. Correlation between the time after daylight adaptation at which the limen was determined and the reciprocal of the stimulus intensity required to evoke just noticeable detail in the Purkinje image.

reciprocals refer to brightness of the slide image in ml. The sensitivity at the discontinuity in rate of change of sensitivity is just below 0.30, which corresponds to approximately 3.4 ml., or 164 photons.

The weighted mean of all determinations made by the 4*O*s is 176 photons.[60] We conclude therefore, that, at a liminal stimulus of brightness close to 180 photons, sensitivity for the sharply-defined Purkinje phase increases suddenly. Troland gives 100 photons as the stimulus-brightness necessary to in-

[57]Fröhlich also determined an adaptation curve for the complementary Purkinje image (*i.e.* a higher intensity in the stimulus will change the gray image to the complementary image, or stage *c*, see Part I, this section). As mentioned before in this connection, 6 of the 8 *O*s reported a stage (*b*) of the Purkinje phase intermediate between the gray stage (*a*) and the complementary stage (*c*), that is, an intermediate whitish-violet stage. For these *O*s an adaptation curve for the complementary stage would logically have to be preceded by a curve for the whitish-violet stage.

[58]Fröhlich, *op. cit.*, 53, 1922, 91.

[59]H. Piper, Ueber Dunkeladaptation, *Zsch. f. Psychol.*, 31, 1903, 161.

[60]Some determinations were given greater weight than others because the corresponding curves showed a more definite break.

sure, after steady stimulation of 10 min. the complete adaptation of the retinal rods to insensitiveness.[61] We assume, then, that 180 photons corresponds approximately to the brightness of a momentary stimulus just low enough to permit functioning of the rods. It is then but an easy step to say, as is suggested by Fröhlich in this connection, that the discontinuity in rate of change of sensitivity marks an appearance of the rods in a rôle in which they were previously absent. The experiment does not, however, serve to identify the new rôle.

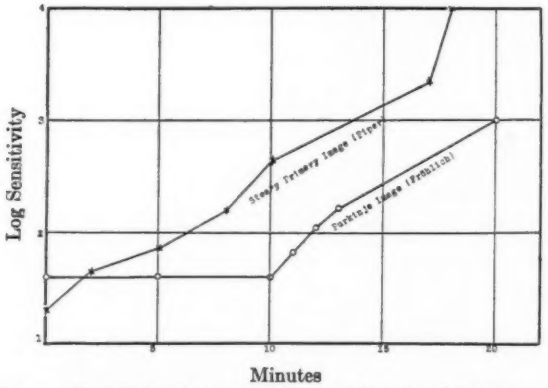


FIG. 4. Correlation between minutes of dark adaptation and the logarithm of sensitivity.

Fröhlich obtained experimental results showing the same discontinuity in rate of change of sensitivity by setting upon the appearance of a separate Purkinje phase instead of (as in the present work) the appearance of a sharply-defined Purkinje image. He interpreted his findings, however, as supporting the known course of dark adaptation for steady vision. He says that the study of the Purkinje phase allows the sensitivity of the visual organ to be studied in a new way, supplementary to the method of Piper,[62] and that the two methods yield the result that adaptation to darkness commences only after 10 min. The discontinuity in the slope of the sensitivity curve for the Purkinje phase after 10 min. adaptation to darkness is not commented upon by him.

If the discontinuity in adaptation rate is caused by the rods, Fröhlich's contradictory conclusion, from essentially similar data, must be incorrect. The curves plotted by Piper show that the sensitivity stays *very low* during the first 10 min. of dark adaptation compared to the heights attained after 20 min. Reference to the table of sensitivities from which the curves are plotted shows that adaptation *is* going on during the first 10 min., and, in some cases, *faster in the first 10 min. than at any other time.*

[61] Troland, *op. cit.*, 555. [62] Piper, *op. cit.*

To make this point evident, the sensitivities obtained by Piper from steady vision have been compared, in Fig. 4, with those obtained by Fröhlich from settings on the Purkinje phase. The logarithm of the sensitivity[63] is plotted against the duration of adaptation to darkness, taking as typical of Piper's data a run in which the *O* was Piper, himself, and as typical of Fröhlich's data on the Purkinje image, a run shown in curve form in Fröhlich's own paper.[64] From Fig. 4 it is evident that the two methods of studying adaptation to darkness measure different processes. The curve of Piper's results shows that the adaptation to darkness, of steady vision, proceeds at a fairly constant rate from the very beginning; the curve representing Fröhlich's results shows plainly the increased rate of change in sensitivity for the Purkinje image commencing at 10 min.

The conclusion suggested by the present work that at a stimulus-brightness of about 180 photons the rods function in the production of the Purkinje image is therefore substantiated by the Fröhlich data in spite of the fact that he does not so interpret it.

*VI. Individual differences.* The fact that in the rapid after-image phases, of which the Purkinje phase is one, we are dealing with phenomena that are less stable and more variable than ordinary vision, deserves special emphasis. The only reason any degree of agreement has been obtained among the different investigators seems to lie largely in the fact that the most striking individual differences are quantitative in character. The failure of the workers to attempt a quantitative specification of stimuli has allowed these individual differences to go unnoticed.

There seems, however, considerable justification for the statement that the same processes occur in most individuals. The stimulus-intensities and durations required to evoke identical processes in two different and supposedly normal, individuals are exactly the same, and are often markedly different. (See summary of results in Fig. 2; the minimal brightness is seen to vary almost in a 100 to 1 ratio between observers.)

There has, however, been some evidence of actual absence of normal processes in certain otherwise normal *O*s. Thus 2 *O*s (H. P. and W. A. H.) failed to see the whitish-violet stage of the Purkinje image described by the other 6 *O*s. It may be pointed out that the same 2 *O*s also gave by far the lowest minimal brightness (Fig. 2) for the appearance of a sharply defined Purkinje image.[65]

A study of the effect of dark adaptation upon the stages of the flash-image was made with 1 *O* only (W. T. M. F.). This study was undertaken because this *O*, in contrast to all the other *O*s, failed to report a Purkinje

[63]The course of dark adaptation, as shown by plotting the logarithm of sensitivity, has already been discussed. Cf. H. v. Helmholtz, *op. cit.*, ii, 1924, 321.

[64]Fröhlich, *op. cit.*, 53, 1922, 91.

[65]It may be mentioned that the bio-luminescence hypothesis (discussed in the section to follow) would require the disappearance of the whitish-violet stage of the Purkinje image for all *O*s exhibiting an extremely high sensitivity to the sharply-defined Purkinje phase. According to this view, then, absence of the whitish-violet stage does not mean the absence of a process but corresponds to a quantitative distinction, namely: an extraordinarily high cone 'fatiguability' attendant on the high cone sensitivity required for discrimination of detail at low brightness levels.

image under certain stimulus conditions. It was found that he saw the Hering phase during the first 10 min. of dark adaptation; and then, when adaptation was carried further, he saw the Purkinje image which was normal to all the *Os* at the stimulus intensity and duration used. In observations taken with the other *Os*, increased dark adaptation functioned as the analogue of increased stimulus intensity; for this *O*, however, light adaptation definitely favored the Hering phase to such a degree that the smallest stimulus brightness that would elicit any flash-image at all, failed to yield the Purkinje phase. It may be possible to connect this failure of the Purkinje phase with the high liminal brightness required by this *O* for eliciting the Purkinje phase after adaptation to darkness, but, what is of immediate significance is the establishment of the fact that individual differences in quantitative results do occur.[66]

## Results: Theoretical

From Bidwell,[67] who accounted for the Purkinje images (the 'ghost' or 'recurrent image') as due to the action of the violet-sensitive fibers of the retina, up to Fröhlich,[68] who compared the entire after-image series to other periodic reactions of the nervous system and considered them as periodic reflexes of perception, many explanations of the phenomena have been proposed. While a few authors have remained discreetly silent concerning theories, still particularly since the work of Von Kries, there has appeared in the literature much argument and speculation as to whether the Purkinje phase was primarily a rod phenomenon (as Von Kries maintained), or a cone phenomenon (as the appearance of colored Purkinje images seemed to indicate).

As noted above, however, there existed much confusion in the identification of the separate phases of the after-image series. Under these conditions it is not surprising that theories concerning the cause of a single phase should be inadequate. Since practically all doubt in phase identification has recently been removed, the chance of arriving at a consistent explanation of the Purkinje image is now much better and a reconsideration of theory seems to be warranted.

It must be pointed out, at the outset, that the principal stumbling block to the theorists has been the fact that although the Purkinje phase was undoubtedly colored under many ex-

[66]Three *Os* (J. P. G.; E. A. S.; W. A. H.) independently reported that the Purkinje image with dark surrounding field seemed to jump closer to the eye than the primary image (*i.e.* seemed to leave the plane of the screen on which the slide-image was focussed and to appear perhaps 4 in. out in the space between the screen and the observing eye).

The same *Os* reported a flash image in some cases which consisted of two colors, one nearer to the eye than the other, as if (W. A. H.) "one were looking at one color through a semi-transparent film having a different color;" or, as if (E. A. S. following an orange primary image) "the green-blue Purkinje phase was suddenly overlain with a violet sheen."

One *O* (W. A. H.) reported consistently the appearance of the Purkinje image followed by the Hering image. This was for the blue primary image only. He reported that the blue phase was too quick to be the third positive phase which remained as a diffuse gray patch for several seconds after the disappearance of the blue flash.

[67]Bidwell, *op. cit.*, 142.

[68]Fröhlich, *opp. citt.*

perimental conditions, and, therefore, seemed to depend on the action of the cones, still, the image often failed to appear in the fovea centralis and always failed to appear for a primary stimulus of pure spectral red. These latter two established facts point to a direct dependence on rod action, rods being not only absent in the fovea centralis but also insensitive to spectral red. An assumption, perhaps as simple as any, which is capable of reconciling this apparently contradictory nature of the Purkinje phase is that it results from a complex process depending both on the cones and on the rods. The present immediate purpose is to apply this assumption to our experimental results.

*I. Identification of the after-image phases.* Three stages in the Purkinje phase are identified as intensity of the stimulus is increased: (a) the gray stage, independent of the hue of the primary image; (b) the whitish-violet stage, also independent of the hue of the primary image; and (c) the complementary stage, in which the Purkinje phase is complementary in hue to the primary image.

These three stages, (a), (b), and (c) may be set in exact correspondence to the stages (a′), (b′), and (c′), respectively, which would be caused from a stimulation of the retina by an hypothetical secondary stimulus of whitish-violet light of increasing intensity following the original stimulus, thus: (a′) the gray stage is due to stimulation of the retina by the whitish-violet light (*i.e.* accurately, light of all wave lengths with the shorter wave lengths predominating in intensity), but the intensity of this light is below the levels of cone sensitivity; (b′) the whitish-violet stage corresponds to all the cases (except c′) where the secondary light is of high enough intensity to affect the cones; (c′) the complementary stage corresponds to stimulus intensities and durations large enough to fatigue the color-sense ascribed to the cones so that the secondary light, which by unfatigued cones would appear whitish-violet, now appears of a hue complementary to that of the primary image, the whitish-violet character having been rendered undetectable because of the instantaneous (since there is not time for precursor action to replenish sensitive substance) fatigue of the cones.

To account for this hypothetical secondary radiation it is necessary to assume that some retinal structure gives off light about 1/5 sec. after the original stimulation. It has been long, if not widely known, that phenomena exist which are easily explained by the assumption of bio-luminescence of retinal structures. The blue-arcs of the retina[69] have been shown by Ladd-Franklin[70] to be due to bio-luminescence of active nerve fibers.

[69] W. R. Amberson, Secondary excitation in the retina, *Amer. J. Physiol.*, 69, 1924, 354-370.

[70] Christine Ladd-Franklin, The blue arcs of the retina, 'Seeing your own nerve currents,' *J. Opt. Soc. Amer.*, 12, 1926, 494.

If we are to assume that the positive character of the Purkinje phase depends on the actual emission of physical light by excited nerve tissue, it seems natural to fasten upon the nerve structure closest to the receptor layer of the retina as the most probable source of the whitish-violet secondary radiation.

*II. Spectral effectiveness.* The effectiveness of a spectral stimulus for producing the complementary stage of the Purkinje phase is approximately determined as a function of wave length. If bio-luminescence of a retinal structure does occur, the effectiveness would depend on the condition of the receptors (rods and cones) before the primary stimulation (production of the nerve activity which causes bio-luminescence) and would also depend on cone fatigue due to primary stimulation (response to the secondary radiation caused by bio-luminescence). The relation of spectral effectiveness to wave length gives information (according to this hypothesis) about the combined effects of: (1) the production of bio-luminescence; and (2) the instantaneous 'fatiguability' of the retinal cones. Without more specific knowledge about the exact retinal structures concerned in bio-luminescence, it seems unwise to attempt a resolution of the combined effect (which is, itself, none too accurately established) into its components.

*III. Distinctness of the images in the different phases.* The distinctness of the Purkinje image is shown to be surprisingly high—certainly no lower than ¾ that of the primary image. If the bio-luminescing structure were far out in the retinal layers away from the receptor layer, a sharp Purkinje image could not arise. The indication is, therefore, that the bio-luminescing structure is close to the light-sensitive layer of the retina.

*IV. Sensitivity, a function of stimulus-duration.* The conclusions indicated by the fact that liminal brightness for the production of a detailed Purkinje image rises both for an increasing to high values of stimulus-duration and for a decreasing to low values must be referred specifically to bio-luminescence during or following the occurrence of nervous activity in a fiber, instead of indefinitely to the process producing the Purkinje phase, as follows:

(1) Bio-luminescence is aroused optimally by a stimulus of 0.16 sec. duration. (2) It is possible that bio-luminescence requires a definite minimal energy of light (or, perhaps, of nerve activity) to release it.

These results do not, of course, constitute a test at all crucial for any detail of the hypothesis.

*V. Sensitivity, a function of dark adaptation.* It is noted that detail appears in the Purkinje image at about the stimulus-brightness which marks the transition from the gray stage (Stage a) to the whitish-violet stage (Stage b). This appearance of detail coincident with the perception of hue substantiates the conclusion drawn from *Part I* that the intensities of the bio-luminescent light which produce the gray stage in the Purkinje image correspond to light intensities below the cone level of sensitivity, and that the intensities of bio-luminescent light producing the whitish-violet stage (Stage b) correspond to light intensities just above this level.

Further, a conclusion is suggested from the discontinuity established in rate of change of sensitivity, that the rods of the retina after about 10 min. adaptation to darkness enter into a new rôle in the production of the Purkinje image. The bio-luminescence assumption now under consideration makes it possible tentatively to name this new rôle. According to the bio-luminescence hypothesis the rods may act in two ways: (1) to produce the nerve activity which causes bio-luminescence; and, (2) to respond again under the influence of the light produced secondarily from their own previous activity. For stimulus-brightnesses corresponding to the flat part of the sensitivity curve (*i.e.* brightnesses greater than 180 photons) the rods are bleached to insensitiveness by the first light pulse, the bio-luminescence due to which is, therefore, perceived only by the cones. For stimulus-brightnesses corresponding to the steep part of the sensitivity curve (*i.e.* brightnesses less than 180 photons) a part of the sensitive substance in the rods remains after the action of the primary light by means of which substance (presumably the visual purple) the secondary light is perceived. If we make this designation of function for the rods, then, not only is the discontinuity in rate of change of sensitivity accounted for, but also, with fair plausibility, the stimulus brightness (180 photons, or about twice the brightness required to bleach the rods to insensitiveness by steady stimulation) at which it occurs.

The assumption of a complex process involving both rods and cones for the production of the Purkinje phase leads, then, to a workable hypothesis, many of the attributes of which have been defined by applying the experimental results of the present work. This hypothesis involves a series of processes which may be summarized as follows:

(1) By the usual direct action of light on the sensitive retinal layer, a photo-chemical reaction takes place in the rods and cones.

(2) Nerve activity results from this chemical change in both cones and rods which leads to two effects:

(*a*) the perception of the usual primary image; and

(*b*) the bio-luminescence of the nerve fibers (or surrounding structures) close to the rods and cones. Since the rods, if not light-adapted, are always much more sensitive to light than are the cones, the nerve activity due to them must be much greater than that due to the cones. Hence, most, if not all, the bio-luminescence causing the usual Purkinje image results from the tremendous pulse of nerve activity following instantaneous bleaching-out of visual purple in the rods.

(3) The secondary radiation produced by the bio-luminescence of the nerve fibers is almost non-selective, the shorter wave lengths being slightly predominating.

This light is perceived:

(*a*) by both rods and cones as the whitish-violet stage (b) or, less likely, the complementary stage (c);

(*b*) by the rods alone, if the bio-luminescence is so weak that it is below the level of cone sensitivity, as the gray stage (a); or

(*c*) by the cones alone, if the primary radiation is intense enough to bleach the rods to insensitiveness, as the completementary stage (c) or, less likely, whitish-violet stage (b).

This hypothetical series of retinal events suffices to explain the results of the present investigation; it is interesting to view a few cases previously reported in which this hypothesis serves to harmonize the contradictions.

As mentioned before, some investigators maintain that the Purkinje phase disappears[71] at and near the retinal region of most distinct vision. Following Fröhlich's excellent treatment of the fact of appearance of the Purkinje phase in the fovea under some conditions and its non-appearance under others, we must conclude: (1) that the sensitivity of the Purkinje phase is much less within the extreme central region (fovea centralis, approximately 1° in diam.) than within any other retinal region; (2) that the sensitivity increases rapidly in the region between 1°–7° in diam. (the macula lutea and the so-called para-foveal region); and (3) that it remains substantially at this high value for the remainder of the retina.

We would expect, according to our bio-luminescence hypothesis, that three retinal regions would be differentiated on the basis of sensitivity.

(*1*) The periphery (all of the retina except a central area of 3°–4° in diam.) should be the region of highest sensitivity because rods are present in large numbers. The sensitivity should decrease slightly toward the fovea because of the decrease in the number of rods compared to the number of cones in the para-foveal region.[72]

(*2*) A semi central zone, comprising the remaining 3°–4° (with the exception of the foveal pit, itself, which is roughly 1° in diameter), should yield a lower sensitivity because of two factors: (a) the proportion of the number of rods to the number of cones[72] continues to decrease—as

[71] V. Kries, (*op. cit.*). Cf. also Samojloff, (*loc. cit.*) who gives the area of the retina insensitive to the Purkinje phase as between three and four degrees. Fröhlich, (Ueber den Einfluss der Hell und Dunkeladaptation usw., *loc. cit.*) gives the area insensitive under certain stimulus conditions for the Purkinje image an extent varying from 4°-7° in diam.

[72] Quain, Anatomy, III, 1897, 47.

[72] The evidence for the presence of rods in the semi-central zone is entirely indirect (based on the Purkinje effect). See Helmholtz, *op. cit.*, 2, 1924, 363. See also Troland, *op. cit.*, 555. There seems to be no direct anatomical evidence of rods within the 3° area. If it should be assumed that this region is rod-free, sensitivity for the Purkinje phase should drop to a very low value on that account alone.

in (*1*)—and would lead to the prediction of a continued gradual decrease in sensitivity; and (b) the absorption of light of short wave length by the macular pigment which overlies much of this zone would lead to the prediction of a still lower sensitivity.

(*3*) The foveal pit, should yield a much lower sensitivity than any other region, because here in the normal *O* there are no rods and no overlying nerve tissue. There are, then, two ways by which a Purkinje phase could become visible in this region. (a) By a light stimulus falling upon the foveal pit, causing the cones there to give rise to nerve activity sufficiently intense to evoke bio-luminescence. This method would presuppose that the cone fiber just within the *membrana limitans externa*[74] is the bio-luminescing structure since so little nerve tissue overlies the foveal pit. (b) By a light stimulus falling upon the retina in the vicinity of the fovea causing bio-luminescence of sufficient intensity to affect not only the retinal elements external to the fovea centralis by whose action it was caused, but also, by scattered light, the cones of the foveal pit which are very close to them. It is seen that the sensitivities arising from either chain of events (a) or (b), would be lower than that characterizing the semi-central zone.

These three retinal regions differentiated on the basis of sensitivity to the Purkinje phase by the bio-luminescence hypothesis are clearly in very close correspondence to the findings of Fröhlich.[75]

There are two very striking experiments reported by Bidwell,[76] a satisfactory explanation for all details of which has not yet been offered.

(*1*) Following a moving spectrum, each separate region of which gave a complementary colored 'ghost,' appeared a 'ghost' (*i.e.* Purkinje phase)

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[74]Quain, *loc. cit.*, 53.

[75]The structural distinction between the fovea centralis and the semi-central zone of the retina seems to give a tool by means of which we should be able to determine with considerable precision the retinal element producing bio-luminescence. Thus, if the fovea, alone, be stimulated by an intense light pulse, a Purkinje phase seen at the fixation point would suggest that method (a) was the true method by which foveal Purkinje phases are produced. If this Purkinje phase were surrounded by a brighter ring more than one degree in diameter, we should conclude that the bio-luminescence had also occurred later in the chain of events than the cone fiber (*i.e.* had occurred in the early neural structures which do not overlie the foveal pit, but do overlie the semi-central zone). This experiment is difficult because of at least the following three factors: (1) Light scattered by the optical media of the eye may be sufficiently strong to arouse a Purkinje image in the semi-central zone in the ordinary way; (2) steady fixation is essential to the success of the experiment, and, if something suggesting a halo were reported, the question of accommodation error and possible failure to control fixation could always be raised; and (3) the Hering phase does not suffer a marked change in sensitivity as the center of the retina is approached and would, therefore, always be much more prominent than the Purkinje phase toward which observation should be directed.

With a moving stimulus, the last difficulty is not present because phase identification is much easier with that method. If we take a slit of light falling vertically across the fovea but a little to one side of the center, and observe the Purkinje phase to be not a straight slit like the primary phase, but showing rather a bending out around the border of the fovea, it might be concluded that method (B) correctly placed the seat of bio-luminescence. If the slit is stationary, the above-mentioned difficulties would be encountered; if moving, then an observed bending out as the fovea was approached could also be interpreted as a longer development time of the Purkinje phase for the semi-central zone. This experiment has, in fact, been tried out by Fröhlich (*loc. cit.*, 53, 79-107); a bending-out ("Ausbuchtung") has been observed by him; and he did place the above alternative explanation on his results.

For these reasons, then, although experiment does not gainsay the bio-luminescence hypothesis, the structural difference between the fovea centralis and the semi-central zone cannot be made use of easily to locate the seat of bio-luminescence, providing, of course, bio-luminescence is actually present.

[76]Bidwell, *op. cit.*, 137, 139.

which was *all violet*.[77] From this observation Bidwell suggests that the 'ghost,' is due to the action of the violet fibers (the violet of the fundamental red, green and violet of the Young-Helmholtz theory of color-vision) of the retina only. He ascribed the complementary phases, previously observed, to errors in color judgment since no basis for color judgment, except the saturated primary image of one hue, was available in the field of vision. It has been established since by many investigators that the complementary character of the Purkinje phase is real and not based upon errors of color judgment.[78] Bidwell's suggestion, must, therefore, be rejected. Since, however, as is common to all investigators before Fröhlich, no specification of stimulus-brightness is made, we think it reasonable to assume that the brightness of the spectrum used by Bidwell was so low that the cones momentarily affected by it were not fatigued. The subsequent hypothetical bio-luminescent light would, therefore, be perceived in its true whitish-violet character. The intensity of the image indicated by the diagram of the 'ghost' spectrum following the primary image of the spectrum is strongly suggestive of rod visibility (as the bio-luminescence hypothesis requires) because it shows a shift in maximum intensity with respect to the primary image toward the short wave lengths.

(*2*) It was further reported that a yellow primary image from a slit illuminated by a mixture of spectral red (which gave no 'ghost') with spectral green (which gave a violet 'ghost') was followed by a blue 'ghost,' exactly as was a yellow primary image evoked by a spectral yellow. According to the bio-luminescence hypothesis, the reason spectral red is ineffective (see Fig. 1) is that it does not affect the rods from which the nerve activity causing the bio-luminescence arises. Spectral red is, however, effective in fatiguing the cones, and will, therefore, play its part in rendering the Purkinje phase complementary in hue to the primary image. The bio-luminescence producing the 'ghost' following the yellow primary image is due entirely to the green component which alone affects the rods; the fatigue of the cones is due to both the red and the green components.

The bio-luminescence hypothesis, therefore, accounts for some specific observations heretofore remaining unexplained. Further, all reliable conclusions concerning the Purkinje phase may be put into terms of the hypothesis. It has, throughout the course of the present work, exhibited all the characteristics of a successful hypothesis. Whether the chain of retinal events making up the bio-luminescence hypothesis or some modification of this chain of events really exists remains to be decided by subsequent experimental work.

## Summary

(1) As the brightness of the stimulus is increased the Purkinje image passes through three characteristic stages: (*a*) gray, (*b*) whitish-violet, and (*c*) complementary to the primary image.

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[77]Bidwell says that the spectrum was moved parallel to itself across the field of vision, which might imply that a given retinal element was affected in turn by light of all wave lengths before the 'ghost' appeared from its action. Subsequent discussion of the results and a diagram of the 'ghost' spectrum make it seem likely that the spectrum was really moved perpendicular to its dimension of greatest length, or in such a way that a given retinal element was affected by light of a single wave-length and, then, without further stimulation exhibited the 'ghost.' If the former statement really represents the experimental procedure, the bio-luminescence hypothesis would require a whitish-violet 'ghost' regardless of the intensity of the stimulus, since each cone would be fatigued successively by light of all wave-lengths, the resulting fatigue effect being, of course, non-selective so that the true whitish-violet character of the bio-luminescence radiation would become evident.

[78]The Purkinje phases of striking complementary character reported in our work, following a stimulus each quadrant of which was differently colored, shows definitely that the complementary character does not arise from lack of a basis for color judgment.

(2) Light of short wave length is more effective than that of long wave length in evoking the complementary Purkinje phase. The effectiveness of spectral light in evoking the complementary stage has been evaluated as a function of wave length.

(3) Under favorable conditions the Purkinje image is not less than ¾ as distinct as the primary image.

(4) The brightness of the stimulus evoking a distinct Purkinje phase is lowest when the stimulus has a duration of 0.16 sec. If the duration of the stimulus is greater than 0.02 and less than 0.30 sec., a distinct Purkinje phase may be aroused by a stimulus whose brightness lies between 200 and 800 photons.

(5) The lowest brightness of stimulus required to evoke a distinct Purkinje phase varies with the observer from 0.60 to 60 photons for a stimulus having the radiating characteristics of a black body at 2790° K.

(6) The semi-quantitative findings of Fröhlich concerning the effect of adaptation to darkness on the Purkinje phase are corroborated. The brightness of stimulus required just to evoke a distinct Purkinje phase is measured in photons as a function of dark adaptation.

(7) Our results suggest that some retinal structure close to the light-sensitive layer gives off physical light about 1/5 sec. after the beginning of the stimulus.

(8) The assumption that a retinal structure exhibits this property of bio-luminescence serves to explain certain apparent contradictions in former results.

# 'FLUCTUATIONS OF ATTENTION' WITH WEAK VISUAL STIMULI[1]

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## I. INTRODUCTION

The problem of 'fluctuation of attention' has been settled and unsettled at least four times in as many generations of experimental psychology.[2] Whether the observed changes in minimal sensory experiences should be attributed to periodic lapses of attention or to extra-attentional factors has been the chief question. For those to whom it means fluctuation the attentional changes have been of two kinds. If we take the two-level theory of attention as our basis,[3] the disappearance of a minimal quality might mean either a change of content of the upper level (a loss of clearness due to a redistribution of vividness in the 'mental content') or it might mean a periodic

[1]From the Psychological Laboratory of Cornell University.

[2]It is a pleasure to make acknowledgment to Professor K. M. Dallenbach who, besides serving as an observer, gave me constant help and counsel on matters of technique and procedure.

[3]E. B. Titchener, *A Textbook of Psychology*, 1910, 276.

loss of the degree of clearness of the upper level regardless of change of content—a periodic loss of efficiency of attention, to put it in functional terms.

The chief questions upon which the outcome has depended are two. (1) Is the phenomenon found in all sense departments? If it is a matter of attention the answer to this question must be in the affirmative. (2) Are the conditions for fluctuation peripheral or central? In order to be a matter of attention it has been assumed that the conditions must be central. For those who hold the first view of fluctuations of attention the conditions may be central but local; for those to whom there is a periodic loss of attentional ability, the conditions must have a general central basis. Other questions have interested the latter group, namely, Why are the fluctuations rhythmical? What determines their rate? These questions have led to attempts to correlate fluctuations with various bodily rhythms.

The history of the problem presents a typical picture of development in experimental psychology. On the side of theory we often find, especially during the early period, the expression "fluctuations of *the* attention"—ghosts of a pre-experimental period; and all the way through we find psychological theory mixed with or opposed to physiological theory. On the side of experiment we find one crucial test after another as laboratory technique has improved. We present the following historical outline, keeping to the chronological order for the greater part; but also having some regard for the development of certain phases of the general problem. Special effort has been made to state the problem as each investigator saw it, to point out his salient results and interpretations, and to refrain from criticism of theory except upon points which cannot be later included in the discussion of our own results.

## History

It was Urbantschitsch,[4] a specialist in aural disorders, who made the first specific study (1875) of the fluctuation of minimal sensations. He had found, in testing the hearing of his patients by means of the ticking of a watch, that the sound would come and go without a change in the distance of the watch from the ear. This phenomenon was not purely physical because when he asked several individuals to report simultaneously the appearance and disappearance of the sound their reports did not coincide. Similar results were obtained with a faint stream of water and with an electrically driven tuning fork. When he examined the ear to find a possible source of the phenomenon there he discovered that patients who had perforated ear-drums and also one patient whose stirrup bone was disconnected had the same experience. The fluctuations could even be perceived when the sound vibrations were transmitted to the inner ear

[4] V. Urbantschitsch, Ueber eine Eigenthümlichkeit der Schallempfindungen geringster Intensität, *Centralbl. f. d. med. Wiss.*, 1875, 625–628.

by bone-conduction. He drew the conclusion, without making any specific tests of it, that the basis for the fluctuations is in the "perceptive ability" of the auditory nerve.

Nicolai Lange[5] (1888) brought the phenomenon into psychological prominence with his experiments on sensory attention, in Wundt's laboratory. He dismissed the fatigue theory of Urbantschitsch with the arguments (1) that there is no evidence that sensory nerves tire so quickly from minimal stimuli, (2) that the nerve would have to recover while being stimulated, and (3) that the effect ought to be noticed with intense stimuli. He then sought to show that the fluctuations have a central origin, in the periodic imaginal reinforcement of the sensation. Using as stimuli the Masson disc, the ticking of a watch, and electrical contacts on the skin, he found slightly different periods of fluctuation, measured from one point of maximal intensity to the next, in the three sense departments. When he asked the *O*s to report upon the fluctuations of the gray ring and the sound presented together, the two did not fluctuate independently at their own rate but formed a new rhythm, gray alternating with sound at definite intervals. This he offers as an argument for a central theory along with the fact that images of the three sensations fluctuate in rates which correlate exactly with those of the original sensations. Lange concluded that these phenomena are fluctuations of active apperception which produces a periodic assimilation of image with sensation.

Münsterberg[6] (1889) followed with a critical and experimental study. At the outset he objects (1) that Lange's *O*s could not report accurately the point of maximal intensity; (2) that Lange's results were too regular and fell too nicely into his theory; (3) that the combined visual and auditory fluctuation is an entirely different phenomenon; (4) that Lange's theory places the source of the fluctuations in the realm of the transcendental. Münsterberg set himself the following problems: to determine whether fluctuation is "a matter of the activity of consciousness and not a matter of changes in conscious content;" and "in how far is it a central matter, and in how far the process is peripherally conditioned."[7] Using 1 *O* besides himself, and the Masson disk as stimulus, he obtained the following results. If the eyes move in order to keep fixated upon a stimulus which is moved, either quickly or slowly, the periods of fluctuation as measured from one moment of disappearance to the next—are noticeably longer, or in some cases the stimulus fails to disappear. Voluntary winks every one or even two seconds prevent a disappearance, although a similar cutting off of the stimulus by placing a gray card over it every one or two seconds even shortens the fluctuation periods. He also found that short periods occurred along with rapid breathing and long periods with slow breathing and that often a disappearance coincided with an inspiration. He concluded that the conditons of fluctuation lie chiefly in the muscles of accommodation and fixation. His theory is that, as the muscles of accommodation become fatigued and relax, dispersion of the contours of the gray ring results and the ring merges into the background. The change in conscious content is followed by a new effort to accommodate and the ring reappears. In the case of fixation, as fatigue sets in, the muscles shift the fixation so that the ring is brought upon a more sensitive part of the retina outside the yellow spot where the gray gains relatively more in brightness than does the white background and hence disappears. Breathing, through widespread muscular contraction and nervous connections, exerts an influence upon accommodation.

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[5] N. Lange, Beiträge zur Theorie der Aufmerksamkeit und der activen Apperception, *Phil. Stud.*, 4, 1888, 390–422.

[6] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 2, 1889, 69–124.

[7] *Ibid.*, 81.

Pace[8] (1893), using the Masson disc, sought to test the results of Lange and Münsterberg. As a test of the 'accommodation' theory he treated one eye with atropin, thus paralysing the muscles of accommodation, and found fluctuations for that eye differing but little from those for the normal eye. He also pointed out that the fluctuations bear some relation to the limen; that as the experimental series progresses the limen is lowered because some of the fainter rings then appear. As the stimulus value rises above the limen during a series, the periods of visibility become longer and the periods of invisibility become shorter and the total period longer. He concluded that eye movements—as in Münsterberg's experiments—may change the fluctuation time by lowering the limen. He suggests that the fluctuations depend upon a reciprocal action of center and periphery.

Eckener[9] (1893), using as stimuli a watch, a Wagner hammer, and a stream of fine sand, attempted to determine whether the afferent nerves carry a constant impulse to the center so long as the physical stimulus is constant. He found two kinds of fluctuations: (1) those of an objective type that are brief, sudden, and complete; and (2) those of a subjective type that are longer, more gradual, and accompanied by an inner effort to bring back the perception. The former he attributes to a failure of the sense organ; the latter to a central cause. He also found that when the sound stimulus was cut off after the *O* had reported a disappearance, the latter was immediately aware of a change in conscious content. He takes this to mean that the afferent nerve is conducting a constant impulse to the center and that failure to perceive therefore has a central origin.

Lehmann[10] (1894) studied the relation between fluctuations and the breathing rhythms. Against Münsterberg he argues that the skin exhibits no muscles of accommodation and the ear shows no fatigue. Using as stimuli a white square on a black background, the hiss from a Bunsen burner and copper electrodes on the skin, he found that only the periods for tactual fluctuations agree in length with the breathing period. Analysis of the other fluctuations, however, showed that most reappearances come a short time after the beginning of expiration and many also occur during an inspiration. He concludes that the most unfavorable times for reappearance are (1) the respiration pause, when the blood pressure is least in the brain, and (2) the maximal inspiration, when neural energy is drained into channels innervating the muscles of breathing. Correlation with tactual fluctuations is best because there are no muscles of accommodation to mask the effect of breathing. The minimal sensation may therefore disappear from fatigue or loss of accommodation, and its return is dependent upon the intensity of the stimulus and upon favorable moments during the breathing cycle.

Marbe[11] (1895), working independently of the Leipzig laboratory, attacked the problem from a slightly different angle. Against Lange's theory he offers the argument that a proof of central origin does not mean that the fluctuations are phenomena of attention, nor does fluctuation of sensation and image in the same periods mean that the one is the cause of the other; they may be traced to a common cause. He maintains that if Münsterberg's theory is true, the stimulus would have to be so weak as to be perceptible only when accommodation and fixation are good, and that only a dark stimulus on a light ground would fluctuate. He dis-

[8]E. A. Pace, Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit Masson'schen Scheibe, *Phil. Stud.*, 8, 1893, 388–401.

[9]H. Eckener, Untersuchungen über die Schwankungen der Auffassung minimal Sinnesreiz, *Phil. Stud.*, 8, 1893, 343–365.

[10]A. Lehmann, Über die Beziehung zwischen Athmung und Aufmerksamkeit, *Phil. Stud.*, 9, 1894, 66–91.

[11]K. Marbe, Schwankungen der Gesichtempfindungen, *Phil. Stud.*, 10, 1895, 620 ff.

proves Münsterberg's first requirement by showing that the gray ring may be varied within relatively wide limits and still give fluctuations. The second requirement he finds not to be true, for a light spot on a dark ground will fluctuate. In varying the gray ring from a point where it was perceptible none of the time up to the point where it was visible all of the time—during a one-minute observation—the following facts came to light: (1) the ring appears first in the periphery and then in direct vision; (2) the periods of visibility lengthen until they equal the periods of invisibility; (3) fluctuations cease last in the periphery as the intermissions become shorter; and (4) fluctuations occur more rapidly near the limen. He draws the conclusion that these fluctuations have the same conditions as those liminal sensations.

The controversy regarding central as against peripheral conditions was carried across the Atlantic when Hylan[12] (1896) undertook a study of fluctuations in the cutaneous field. He used stimuli which gave cold and pressure. When cold and pressure were given together he found that the two could fluctuate independently—a result contrary to that of Lange. As in the experiments of Marbe, he found that an increase in the stimulus produced changes in the periods of fluctuation. As the temperature was lowered the pauses (absence of cold) became shorter; but as the pressure was increased the pauses became longer.

There occurred at this point a many-sided debate over the special question as to whether fluctuation of sound is a fact. It began with Heinrich[13] (1898), who found no fluctuations when pure tones, from a Galton whistle, organ pipes or glass tubes, were used. Cook [14] (1900), in the Cornell laboratory, tested the question with tones from a Politzer acoumeter, a tuning fork, and a Stern variator and found fluctuations with four observers. After further objection from Heinrich that the tones were not pure, Titchener[15] (1901) reported that no fluctuations were found with a gas harmonica and 2 *Os*. Dunlap[16] (1904) tested Heinrich's results by means of tones from a singing flame and diapason tones and found distinct fluctuations with 5 *Os*. He came to a conclusion opposite to that of Heinrich: namely, to the conclusion that fluctuations are favored by pure and simple tones and that complex tones fluctuate because we select out one dominant. Hammer[17] (1905) came in with the suggestion that fluctuating sounds are physical phenomena. Most investigators had used watches or similar instruments in which there is adequate opportunity for periodic changes in physical intensity of sound and his own observations—apparently with no other aids than the human ear—show such changes. Using a ticking electromagnet and 5 *Os* he found no fluctuations. Seashore[18] (1905) in a critique of Hammer reports that with a Runne-chronometer and 55 unpractised *Os* he found fluctuations in every case. The discussion ended about where it began, with Heinrich[19] (1907) and with a restatement of his position: that pure tones do not fluctuate whereas noises do, and that some *Os* report a fluctuation of the noise component

[12] J. B. Hylan, Fluctuations of the attention, *Psychol. Rev.*, 3, 1896, 56 ff.

[13] W. Heinrich, De la constance de perception des tons purs à la limite d'audibilité, *Bull. internat. l'Acad. des. sci. de Cracovie*, 1900, 37 ff.

[14] H. O. Cook, Fluctuation of the attention to musical tones, this JOURNAL, 11, 1900, 119 ff.

[15] E. B. Titchener, Fluctuations of the attention to musical tones, this JOURNAL, 12, 1901, 595.

[16] K. Dunlap, Some peculiarities of fluctuating and of inaudible sounds, *Psychol. Rev.*, 11, 1904, 308–317.

[17] B. Hammer, Zur experimentellen Kritik der Theorie der Aufmerksamkeits-schwankungen, *Zsch. f. Psychol.* 37, 1905, 363–376.

[18] C. E. Seashore, Die Aufmerksamkeitsschwankungen, *Zsch. f. Psychol.*, 39, 1905, 448 ff.

[19] W. Heinrich, Über die Intensitätsänderungen schwacher Geräusche, *Zsch. f. Sinnesphysiol.*, 41, 1907, 57 f.

independent of the pure tone, which remains constant. He submits the evidence from physiology that the ear-drum had been found to make adjustment for noises but not for pure tone. But even for noises the adjustments are noted only when there is a special effort made to hear. Several investigators had reported fluctuations of sounds during this period without entering into the controversy. Among them were Bonser[20] (1903), Wiersma[21] (1901) and Seashore and Kent[22] (1905).

Another paper, important for the controversy, but apprently unnoticed by those who were engaged in it, was presented by Jackson[23] (1906). He used a telephone in which the current was interrupted by means of a König fork of 50 v.d. In order to test the constancy of physical energy in the tone, the amplitude of the vibrations was measured by means of a beam of light reflected from the diaphragm of the telephone and seen magnified in a telescope with a micrometer eye-piece. No change in amplitude which could account for the auditory fluctuations in 6 *Os* could be found. The author states, however, that fluctuations were hard to get; requiring from 5 to 6 days' practice with some *Os*, because the range of intensities which gives fluctuations of sound is extremely limited. *May this not be the reason for the failure of some investigators to get auditory fluctuations at all?* Possibly the proper intensity was not found.

A similar controversy arose over the fluctuations of touch. Wiersma[24] (1901) found that pressures from areal stimuli on the back of the hand will fluctuate, and that, furthermore, these fluctuations obey the same laws as those in vision and audition. Marbe had shown that fluctuations occur over a relatively wide range of stimulus intensity in vision. Wiersma (with Heymans as observer) showed that the same holds for pressures. Ferree[25] (1906) found no fluctuations of tactual sensations either from cork wafers on the arm or from electrical stimulation on the tongue. Geissler[26] (1907), using weights similar to those of Wiersma, found no fluctuations with 4 *Os*, but with 2 other *Os* fluctuations occurred at first. Using a new set of weights much lighter than Wiersma's, the liminal areal pressure would disappear after 2–3 min. and not return, or continue as a vague, indefinite, and unpleasant irritation.

Another phase of the history took place under Pillsbury's direction in the Michigan laboratory. This is the phase which has to do with the correlation of fluctuations with the various bodily rhythms. These investigations began with the assumption that the weight of the evidence up to that time favors a central theory. Slaughter [27] (1900) began the investigations, expressing dissatisfaction with Lehmann's too "general and indefinite" theory and with the inadequate physiological theories of attention and apperception. Using the Masson disc and recording fluctuations over a period of several minutes, he found 3 rhythms in the 'attention wave.' Besides the coming and going of the gray ring as previously reported, he noted a secondary rhythm 10–15 sec. in length and a third rhythm of

[20] F. G. Bonser, A study of the relations between mental activity and the circulation of the blood, *Psychol. Rev.*, 10, 1903, 133 ff.

[21] E. Wiersma, Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen, *Zsch. f. Psychol.*, 26, 1901, 168–200.

[22] C. E. Seashore and G. H. Kent, Periodicity and progressive change in continuous mental work, *Psychol. Monog.* 6, 1905, no. 28, 47 ff.

[23] G. L. Jackson, The telephone and attention waves, *J. Phil., Psychol., etc.*, 3, 1906, 602–604.

[24] *Op. cit.*, 187.

[25] C. E. Ferree, An experimental examination of the phenomena usually attributed to fluctuation of attention, this JOURNAL, 17, 1906, 81–120.

[26] L. R. Geissler, Fluctuations of attention to cutaneous stimuli, this JOURNAL, 18, 1907, 309.

[27] J. W. Slaughter, The fluctuations of the attention in some of their psychological relations, this JOURNAL, 12, 1901, 313–332.

60–80 sec. These rhythms show themselves as an increase and decrease in the rapidity of fluctuation and in the ratio of visibility to invisibility. Rather general correlations with bodily rhythms are shown in the following facts. (1) Pressure exerted by the *O* upon a dynamograph fell during the invisibility period and rose during the visibility period. (2) The periods of fluctuation shortened during exertion and the visibility periods became relatively longer. (3) One *O* showed very short, regular fluctuations and also rapid breathing rate. (4) Another *O* showed abnormally long periods and also long Traube-Hering waves. More direct correlations were found in records where there was an increase of blood pressure during visibility. Two *O*s showed 83% and 91% correspondence of this sort. Slaughter concludes that the fluctuations may be correlated with 3 bodily rhythms: breathing; Traube-Hering waves; and other longer, still unknown, waves. His theory traces the correlation to nervous connections, not to increased blood-supply in the brain, because the blood-pressure in the brain stands in inverse relation to that in the limbs, and because the breathing and pulse rhythms appear in the record for blood-pressure. The nervous connection is made, he assumes, when the motor discharges for breathing or changes in blood-pressure reinforce the sensory centers. In the same way the motor discharges during voluntary exertion facilitate the sensory centers as shown in the dynamographic and fluctuational records.

Taylor[28] (1900) attempted to study the effect of secondary stimuli upon the fluctuations. In some cases the 'attention wave' was lengthened and in others it was shortened so that no general conclusions could be drawn. Bonser[29] (1903), working independently of the Michigan group, found a confirming correlation with the Traube-Hering waves. He states in conclusion that "the minimal sound occurs soon after the period of vaso-constriction; it increases until the maximum is reached just before the beginning of vasodilation and is lost near the middle of the curve's ascent." Pillsbury[30] (1903), in seeking a measure for fatigue, secured fluctuation-records at three periods during the day. Results show characteristic changes in length of fluctuation and the ratio of visibility to invisibility for the individual *O* throughout the day. Galloway[31] (1904) found that secondary stimuli that affect the fluctuations affect the Traube-Hering waves in a similar manner and that the Traube-Hering waves show the same diurnal variations as Pillsbury had found for the same *O*s.

This chapter also finds its close in the laboratory where it started. After standing for two decades it was taken up again in 1924 by Griffits and Gordon.[32] Preliminary experiments show a slight tendency for the appearance of a memory-image or a liminal light to appear during the trough or upward slope of the Traube-Hering wave. But strange to say the authors found the same tendency when the stimulus itself was made to fluctuate. No correlation was made between the length of the 'attention wave' and that of the Traube-Hering wave either in individual differences or with different stimuli. One is forced to the conclusion that if there is any correlation at all between the two waves, the question as to which is the 'cause' of the other is still open.

The fluctuations in vision, which no one disputes, have retained a more unbroken interest up to our own time. A number of theories have been advanced and have been tested along with the 'accommodation' theory of

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[28]R. W. Taylor, The effect of certain stimuli upon the attention wave, this JOURNAL, 12, 1901, 213 ff.

[29]Bonser, *op. cit.*, 120 ff.

[30]W. B. Pillsbury, Attention waves as a means of measuring fatigue, this JOURNAL, 14, 1903, 277 ff.

[31]C. E. Galloway, The effect of stimuli upon the length of the Traube-Hering waves, this JOURNAL, 15, 1904, 512 ff.

[32]C. H. Griffits and E. I. Gordon, The relation between the Traube-Hering and attention rhythms, *J. Exper. Psychol.*, 7, 1924, 117 ff.

Münsterberg. Slaughter tested this theory by using an *O* who had had a crystalline lens removed and found fluctuations nearly normal in period.[33] Ferree[34] (1906) found fluctuations with two aphakial *O*s. McDougall[35] (1903) has shown in a study of retinal rivalry and of ambiguous figures and of negative after-images, however, that attempts to accommodate or to converge the eyes result in a reinforcement of the sensation for the eye, either under normal conditions or even when it has been atropinized. Heinrich and Chwistek[36] (1907) have shown from the observation of changes in the crystalline lens, made with an ophthalmometer during fluctuations of a small black dot on white, that disappearance of the dot coincides with a change in the lens. These changes are not fatigue effects because they occur from the very first moments of fixation. A point placed beyond the range of accommodation, with myopic *O*s still gives fluctuations. They attribute the disappearance of the dot, which may be as large as .2 cm. in diam.—the larger it is the longer is it visible during fluctuations—to a loss of *Sehschärfe*. In reply to this contention, Ferree[37] (1913) objects (1) that the fluctuation of a point stimulus is a special case never before considered under the subject of fluctuation of attention; (2) that their method of observation of changes in the lens was unreliable, chiefly because eye-movements may produce the same changes in the reflected images seen in the ophthalmometer; (3) that all the facts which they present require no 'accommodation' theory but can be equally well explained on the basis of a theory based upon adaptation and eye-movement; and (4) that it is difficult to see how changes in accommodation can be effective beyond the range of accommodation. He shows by the use of four aphakial *O*s that there are fluctuations of a point stimulus without changes in accommodation and that these fluctuations obey the same laws which he found to hold for stimuli of larger areas in an earlier study, a study which we shall presently review. We may conclude, in the light of the evidence on both sides, that accommodation has its effects upon visual fluctuations but that there would be fluctuations independent of these effects.

Pace[38] (1902) is the first to offer, in a second paper on the subject, a new peripheral theory. He suggests that retinal adaptation has something to do with disappearance of a visual sensation. Adaptation was studied as usual through its after-effect, the negative after-image. He found that if the stimulus is cut off at the moment of disappearance a distinct negative image appears; but if the stimulus is not cut off until the moment of reappearance, the after-image is weak if present at all. His theory is as follows. (1) The light disappears by fatigue of the retina; (2) attention is aroused by this change in mental content; (3) this causes accommodation to lapse; (4) the retina has a chance to recover; and (5) the light reappears as accommodation is restored. Statement (3) is based upon the fact offered by Heinrich that when attention is distracted from the visual object accommodation lapses.[39]

Hammer[40] (1905) adopts adaptation as a means of disappearance, but wishes to substitute shifts in fixation to account for reappearance. He maintains that accommodation alone cannot be responsible for the recovery

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[33]Slaughter, *op. cit.*, 331.
[34]Ferree, *op. cit.*, 94.
[35]W. McDougall, Physiological factors of the attention process, *Mind*, 12, 1903, 473 ff.
[36]W. Heinrich and L. Chwistek, Über das periodische Verschwinden kleiner Punkte, *Zsch. f. Sinnesphysiol.*, 41, 1907, 59-72.
[37]C. E. Ferree, Fluctuation of liminal visual stimuli of point area, this JOURNAL, 24, 1913, 378–409.
[38]E. A. Pace, Fluctuations of attention and after-images, *Phil. Stud.*, 20, 1902, 232–245.
[39]W. Heinrich, Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zsch. f. Psychol.*, 9, 1896, 362 ff.
[40]Hammer, *op. cit.*, 363 ff.

from adaptation because fluctuations occur and must have the same effect. Using adjacent rectangles of gray, giving five different steps or degrees, he finds two indications of adaptation: (1) that during the course of the experiment the visibility time decreases; (2) that a negative after-image can be observed especially with the greater stimulus differences. The only evidence adduced to show the effects of re-fixation is that voluntary shifts in fixation will bring back the stimulus difference.

During 1906 Ferree[41] made two studies in the Cornell laboratory intended to show that visual fluctuations are a phenomenon of retinal adaptation rendered intermittent by eye-movements. The evidence comes from various sources. (1) A stimulus which is physiologically constant—produced by electrical stimulation of the retina—does not give fluctuations. That the stimulus was liminal, however, is doubtful for it took 30 sec. for complete adaptation. (2) The same conditions of stimulation have the same effect upon adaptation as upon fluctuation. Square stimuli within the range of 2 x 2 cm. to 22 x 22 cm. will fluctuate; within this range of areas adaptation also was intermittent. Hues which require a longer time for complete adaptation also show a longer visibility-time during fluctuations. Certain combinations of stimuli and backgrounds which require longer adaptation-time also remain visible more of the time during fluctuations. (3) Eye movements break up adaptation because most *Os* show an excess of eye-movements in the horizontal direction as detected by the movement of a projected after-image and for these *Os* vertical strips fluctuate more than do horizontal. In a second study[42] (1908), Ferree proceeds on the assumption that the fluctuation of negative after-images is the same kind of phenomenon as the fluctuation of sensations. That may be admitted if we overlook the continuous action of a physical stimulus in the case of sensations. He presents the addition to the 'eye-movement' theory that the physiological stimulus is rendered intermittent by streaming phenomena on the retina which are facilitated by eye-movements. The evidence for this theory comes from his study of fluctuating after-images and lies in the following facts. (1) If the stimulus is too small no fluctuations occur; if too large it fluctuates in parts. (2) Good fixation decreases fluctuation; poor fixation facilitates it. (3) Narrow strips fluctuate more than do squares of the same area; they also favor poor fixation. (4) Vertical strips fluctuate more than horizontal; most eye-movements are in the horizontal direction. (5) Voluntary eye-movements (12 cm. to the right every 3 sec.) are followed by a disappearance. (6) Streaming phenomena were observed and also the fact that they are accelerated by eye-movements. (7) In a still later report the streaming phenomena were observed to wipe out parts or all of a negative after-image in the form of a vertical strip.[43] Ferree states his theory as follows.[44] "(1) The intermittence of minimal visual sensation is a phenomenon of adaptation. (2) Adaptation is rendered intermittent chiefly through the influence of eye-movement. (3) Eye-movement interferes with adaptation in three ways: (a) it decreases the total time of stimulation; (b) it gives opportunity for restoration; and (c) more immediately, it determines or influences the washing or streaming over the retina of some material capable of directly affecting the visual processes."

This theory was apparently undisputed until Liddell[45] (1919) in the Michigan laboratory undertook to photograph the eye during fluctuations in order to get simultaneous records. Among 7 *Os* he found two distinct

[41] C. E. Ferree, *op. cit.*, 17, 1906, 81–117.
[42] C. E. Ferree, The intermittence of mimimal visual sensations, this JOURNAL, 19, 1908, 58–130.
[43] C. E. Ferree, The streaming phenomenon, this JOURNAL, 19, 1908, 484 ff.
[44] C. E. Ferree, *op. cit.*, 19, 1908, 129.
[45] H. S. Liddell, Eye-movement during the fluctuation of attention, this JOURNAL, 30, 1919, 241–252.

types. (1) Those with frequent eye-movements during fluctuations, either evenly distributed over the periods of visibility and of invisibility or found mainly during the periods of invisibility. (2) No eye-movements, or very few and at random. In the first group, rarely did an eye-movement coincide with a reappearance or disappearance, and more than one movement could occur without producing an immediate reappearance or a disappearance, and this in spite of the fact that the *Os* were convinced that eye-movements, especially the 'searching movements,' brought back the spot of light. The author concludes that "fluctuations of attention are not causally dependent upon eye-movements."

The possibility of a correlation between changes in size of the pupil was not considered by anyone until Troland[46] (1921) suggested it in a study of adaptation. He found that there were rhythmical variations in size amounting to 2 mm. in diam. and that with stimuli of "moderate intensity" a disappearance was very often immediately preceded by a contraction of the iris. With an artificial pupil there were almost no disappearances of supraliminal lights. The relation of pupillary changes to fluctuations with a liminal stimulus, however, will still have to be determined.

We offer no more general conclusions than the few we have ventured so far. One general criticism might be made. It has been the tendency in most cases to look for one and only one condition of the fluctuations. Such a result would of course be a simple solution. But the whole problem on the physiological side appeals to us as very complicated. The problem, let us also say, appears double, or even triple, and this has not always been recognized. There is a psychological question, which is a systematic one; and there is a physiological problem, which lies, for psychology, in the realms of theory. The systematic question is whether the fluctuations are fundamentally a matter of change in clearness or vividness. It involves the whole matter of the relation of intensity to clearness. The fluctuations may be phenomena of attention regardless of where the physiological conditions may be found; the final test is one of psychological observation. The physiological problem is to ferret out in the organism the necessary and sufficient conditions for the experiential facts. The correlation of the physical and physiological conditions with the various phases of the phenomena of fluctuation constitutes the psychophysical problem. We shall begin our study with this last problem, testing for some of the peripheral and central conditions and for the dependence of fluctuation upon stimulus conditions, and then use as much phenomenological description as we have toward a solution of the systematic problem.

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[46] L. T. Troland, The colors produced by equilibrium photopic adaptation, *J. Exper. Psychol.*, 4, 1921, 362–390. Professor Troland's paper was found after this study was completed, hence the relation between pupillary changes and fluctuations was not examined.

## II. Fluctuations Regarded as a Function of Stimulus-Intensity

One striking thing in the history of the problem is the dependence—first pointed out by Marbe—of the fluctuations upon the intensity of the stimulus. It is also noteworthy, and almost inconceivable, that the phenomenon has been studied without knowledge of the limens of the *O*s and the relation of the stimuli to the limen. Nowhere do we find a specific liminal value given and even Marbe assumed that the stimulus-value which gave most rapid fluctuations was the liminal stimulus without finding that value by any of the customary means of determining limens. We therefore proceeded first of all to find the liminal stimuli for our *O*s.

*Observers.* The *O*s who took part in the preliminary experiments were: Dr. L. B. Hoisington (Ho), assistant professor of psychology; Dr. C. A. Dickinson (Di), now professor of psychology, University of Maine; Mr. George Kreezer (K), assistant in psychology, and Mr. M. K. Walsh (W), fellow in psychology. From these *O*s we have results which bear chiefly on the phenomenal aspect of the problem.

The *O*s who served in the main experiements—one hour at the same time of day, six days a week, throughout the summer of 1926— were: Dr. K. M. Dallenbach (D); Dr. Harry Helson (H), now assistant professor of psychology, University of Kansas; Mr. D. G. Griffin (G), assistant in psychology; and Mrs. Florence Hazard (Ha), scholar in psychology.

*Determination of the limens.* Since we were to work under progressive dark adaptation the liminal value had to be determined for every 5 min. period during the observational hour. We found, however, that if *O* did nothing for the first 10 min. in the dark room, the limen was fairly constant from that time on, especially after 15 min., as is generally known.[47] *O* always went into the dark room after being about the laboratory for some minutes. For about 3 min. after entering the dark room he was subjected to the indirect, diffuse illumination from two electric lights; then the room was entirely darkened. The starting point for dark adaptation was thus about the same from day to day.

The stimulus used here, and in the experiments on fluctuation which follow, was a circular white patch of light 42 mm. in diam. and 170 cm. distant from the eye of the *O*. It was presented in parafoveal vision, since *O* fixated a red dot 2.5 mm. in diam. about 40 mm. above it. The stimulus-light was provided by the Nagel adaptometer[48] in which it is possible to vary the light intensity one part in eight million and to read the stimulus value from a scale. The light source in the adaptometer, three 50 w. lamps, was on alternating current the voltage of which was measured at every hour and found to vary not more than 1 v. in either direction from a normal of 112 v. in any experimental hour, and not more than 3 v. from day to day. The fixation light was furnished by a 2.5 v. flashlight bulb behind a red glass filter and a ground glass, and reflected in a mirror so as to appear to *O* to be just above the stimulus light. The current for the fixation light was about 1 v. and was kept constant by means of a rheostat.

[47] There is one remarkable exception in the case of Ha. Her sensitivity increased at the same rate up to 40 min. of dark adaptation. This indicates a visual peculiarity which the same observer exhibited throughout the experiments on fluctuations.

[48] For a description of the adaptometer, see W. Lohmann, *Zsch. f. Sinnesphysiol.*, 41, 1907, 304 ff.

Tests with a sensitive galvanometer show that changes in the current never exceeded a half milliampere during an observation. It was found important to keep the fixation light constant because of Heymans' law of inhibition; a slight change in intensity would cause the stimulus-light to appear or disappear. The fixation point was regarded as necessary to keep the factor of local adaptation under control, also the factor of greater peripheral retinal sensitivity. Red was used because under dark adaptation it has the greatest attraction for the fovea.[49]

The method of securing a liminal value was a combination of the method of limits and the method of average error, a method used by Nagel.[50] The stimulus was shielded from one eye of *O*, but the fixation light was exposed to both. The limens were found for each eye separately, because the later work was to involve monocular vision.

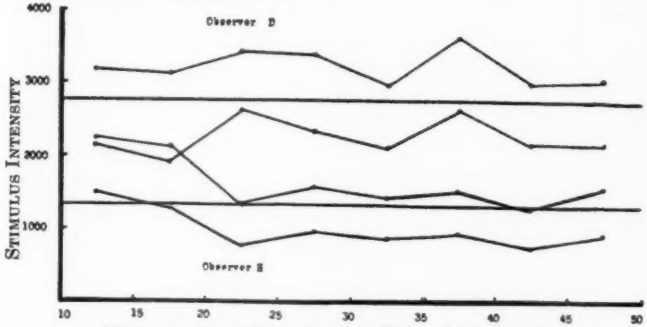



Fig. 1

Three days were given to each eye after two days of practice. The *O*, with his head held in one position by means of a head-rest, varied the stimulus, increasing its intensity until it became visible. He then stopped and reported it 'in.' He then turned a little beyond this position; then reversed the direction, decreasing the intensity until it disappeared, when he would report it 'out,' and so on. *E* took the readings from the adaptometer scale. The readings were begun 11 min. after the beginning of dark adaptation, continued for 3 min., then 2 minutes' rest, and so on throughout the hour. About 8 readings, both 'in' and 'out,' could be made in every 5-min. period with 2-min. rests between.

The curves (see Figs. 1 and 2) show the limits, 'out' and 'in,' for every 5-min. period and the average liminal value for the hour during six days. From them we selected the stimuli to be used in the experiments on fluctuation, using the average liminal value for the whole hour, also the average values of the upper and lower limits as representative stimuli above and below the limen. For 2 *O*s (D and Ha) the limens for the two eyes

[49]For a discussion of fixation in the dark, see Tigerstedt's *Handbuch der physiologischen Methodik*, III, 2 Abth., 1914, 17–25.

[50]For Nagel's discussion, see Tigerstedt's *Handbuch*, 21 f.

were almost identical. For the other 2 *Os* (H and G) there was an ocular difference in one case almost equal to the mean variation for either eye. For that reason we chose 4 stimuli for these two *Os*, but later work showed that the differences were probably due to chance and so we present a single liminal value for them also.

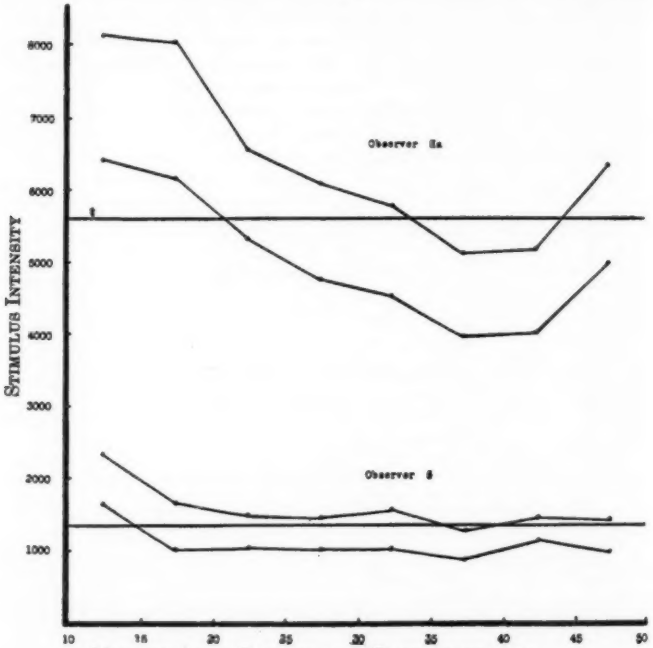



Fig. 2

Whether the limens remained the same throughout the 6 weeks of experiments we do not know. It is reasonable to assume that if improvement in sensitivity or in ability to observe did occur, it took place more rapidly during the first 8 days when we were establishing the limens. We did test for the limen of one *O* (D) later in the term and found that his limen was unchanged.[51]

[51]It is interesting to compare these results with those of Seashore and Kent (*op. cit.*), which they secured in a similar manner with continuous observations lasting 2 hrs. The 'hour' and 'minute' waves which they found may be found also in our data, as a rising and falling of the limen, apart from the initial drop due to dark adaptation. The waves are masked in the curves presented because the latter represent averages for different days. There are, however, other than periodic factors producing a rise and fall in sensitivity; *e.g.* the tendency for the field of vision about the fixation-point and the stimulus to fill up with mists, hazes, and colors. This condition is always accompanied by a higher limen.

*Fluctuations with selected stimuli.* The arrangements for this part of the experiment were exactly the same as in the determination of the limens except that a biting-board was added to the head-rest to give greater stability to the head. *O* was asked to report fluctuations—by pressing a key at the moment of disappearance and lifting the key at the moment of appear-

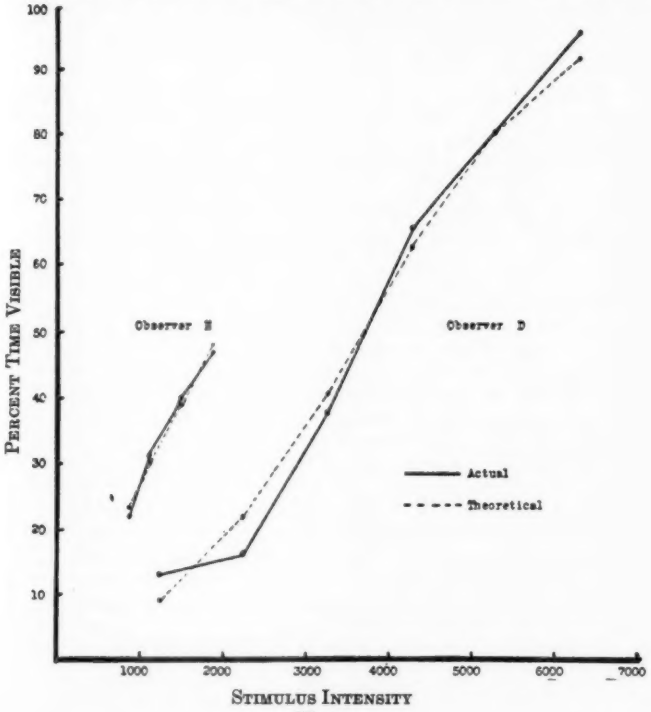



Fig. 3

ance—for 3 min., and then he was allowed to rest 2 min., as before. He was encouraged to take a passive rather than an active attitude toward the stimulus light and to keep fixated upon the red point. The stimulus was set at one of the selected values and left there for the whole hour.

*Procedure.* The recording apparatus was in a room next to the dark room so that *E* and *O* could be in communication at any time. During the 2-min. rest, *O* gave his verbal reports into a dictaphone while *E* prepared

the apparatus for the next series. No records were accepted for later computations until after 2 days of practice in recording fluctuations. Control experiments were interposed from time to time in which *E*, by pressing a key, could cover the stimulus by means of a shutter under electromagnetic control. This served as a check on whether *O* was reporting changes immediately, and in one case it led to an interesting phenomenon which will be mentioned later. Four days were given to every one of the selected stimuli, but other kinds of series—to be described later—were alternated with the simple fluctuation series so that we may say that every stimulus value was presented at least twice, once to each eye, at every 5-min. period during the experimental hour. The kymographic records were timed in sec. by means of a Jacquet chronograph.

*Results*: *Relation to the phi-gamma hypothesis.* Our first problem is to see what fraction of the time every stimulus is visible during the 3 min. of fluctuation. The curves in Fig. 3 show the matter graphically for 2 *O*s. The curve for G coincides almost exactly with that for H, so nearly that it cannot be shown on the same diagram. This is significant because the limens for H and G are almost the same, as may be seen from Figs. 1 and 2. The case of Ha is again peculiar. All stimuli gave visibility approximately 70% of the time. Since her adaptation never reached a point of equilibrium during the hour, as is true for the other *O*s, we might not expect her fluctuations to vary in the more precise manner of the others. We found only too late that the stimuli which were chosen were too near together to be representative. In a few cases where this fact was obvious before calculation of the results, we added one or two more, above or below those chosen as the case required. In only one case were we able to extend the range nearly to the limits in both directions. This was in the case of the most practised observer (D). With the data we have, however, we shall attempt to test Marbe's theory that fluctuations and the limen are fundamentally the same phenomenon. Two questions will constitute our test. (*1*) Does the curve for percent visibility against stimulus-intensity follow the phi-gamma function?[52] (*2*) Does the 'fluctuation' limen, *i.e.* the point of 50% visibility, coincide with the limen found by regular methods?

(*1*) In answer to the first question we call attention to the curve for D which has all the appearances of the phi-gamma function. The dotted line represents the best-fitting theoretical curve. If we compute the goodness of fit of this curve to the theoretical curve, according to the method of Pearson,[53] and

[52] E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280 ff.; also F. M. Urban, *Application of statistical methods to the problems of psychophysics*, 1908, 106 ff.

[53] K. Pearson, *Tables for statisticians and biometricians*, 1914, xxxi. This is only a rough measure of normality of distribution of the data in this case. Thomson's method (see G. H. Thomson, Criterion of goodness of fit of psychophysical data, *Biometrika*, 12, 1919, 216), which has been devised specifically for curves of this type, could not be used because we do not know from these continuous observations the number of judgments.

also of the segments of the curves for H and G, the Pearson P is equal to .266, .976, and .545 for D, H, and G, respectively.[54] All these coefficients are good and in the best case P indicates almost perfect fit.[55]

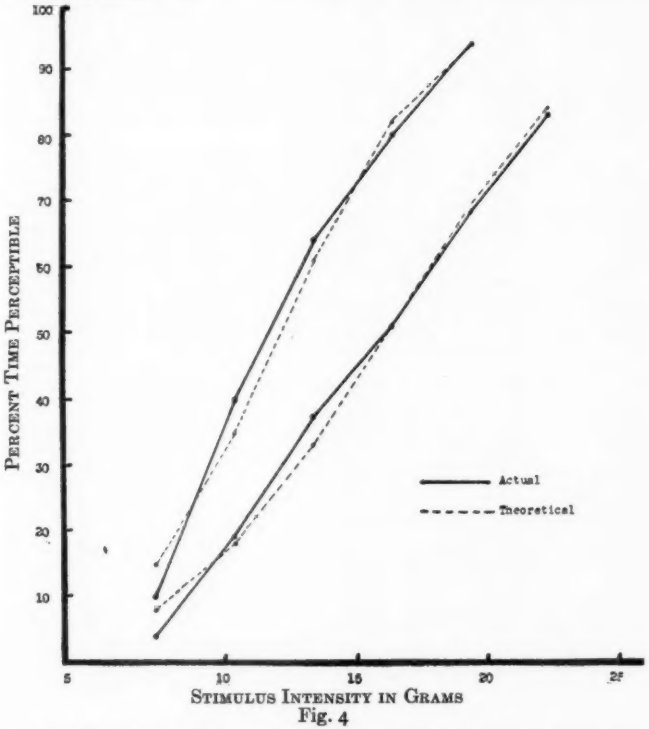



Fig. 4

If we take the liberty of using Marbe's data[56] and treating them as we have our own, we find curves for 2 *O*s which also have the familiar ogive form but which are nonsymmetrical and which fit very poorly the theoretical curves. From data presented by Wiersma[57] we also find curves

[54]Ha's curve is an exception. We have already seen that she is unique in her rate of dark adaptation (see Fig. 1). Control experiments show that we are dealing with a different phenomenon in her case, for when the weaker stimuli were actually cut off she continued to report fluctuations in the same rather regular manner for as long as a half-minute. It is hard to say what was being reported here, whether it was one of the patches of mist or haze, or the positive and negative phases of the after-image of the stimulus, or whether it was a memory image. If it were a memory image we might call it evidence for Lange's theory of imaginal reinforcement; but we cannot be sure that it was.

[55]Cf. E. Culler, Studies in psychometric theory (*J. Exper. Psychol.*, 9, 1926, 286), for interpretation of the Pearson P coefficient.

[56]Marbe, *op. cit.*, 631. [57]Wiersma, *op. cit.*, 180 ff.

which appear to follow the phi-gamma hypothesis. These data come from tactual and auditory stimulation as well as from visual. The Pearson P for the goodness of fit of the curves from visual stimuli are .03 in one case and .35 in another; for the curves from tactual stimuli, .59 and .71. The curves from auditory stimuli appear from inspection to fit even better, but since unequal stimulus-steps were used the data could not conveniently be treated by statistical methods.[58] We present the curves for Wiersma's data from tactual stimulation, with the theoretical curves, in Fig. 4. Marbe and Wiersma were dealing with the difference limen. The former, it will be remembered, was using the Masson disk and the latter differences of tint made Hering grays. We may therefore conclude that fluctuations of a liminal difference obey the same laws as fluctuations of a light at the stimulus limen.

We may predict that the phi-gamma function for fluctuations of sound will have a greater precision (h) due to the absence of sensory adaptation. This is apparently true in the work of Jackson,[59] who found that the range of intensities which give auditory fluctuations is very narrow and hard to determine. We may also venture to account for Heinrich's failure to find fluctuations for pure tones on this same basis. He states that for noises the ear makes muscular adjustments whereas for pure tones it does not. With pure tones, then, still another factor for fluctuations has been removed and the range of intensities which will give fluctuations is even more narrow and hard to find. More work should be done in which the composition of the tones is known and can be correlated with the precision of the psychometric function.

(*2*) The answer to our second question undeniably points to the fact that local adaptation plays a rôle in fluctuation. The limen for fluctuation is in every case greater than the limen found by the method of limits. Using the data obtained in finding the limens we can find the percentage of times each of the chosen stimuli was perceptible and treat the data by the method of constant stimuli.[60] It is obvious that there was also some local adaptation during the tests for the limen, because the stimulus was continually acting upon the same region of the retina. The effect of adaptation should be less than that during fluctuations, however, because the intensity of the stimulus was constantly being changed and *O* was able to shift fixation and to give the eye momentary rests between settings of the stimulus light. We have an opportunity to estimate the liminal value which is independent of local adaptation from data obtained in later experiments. In these experiments the same stimulus light was suddenly exposed to a new region of the same retina lying the same distance from the fovea. Taking the cases where *O* reacted to the light (*i.e.* reported it 'present' with his key) within one second (the average reaction time was about 1.00 ± 0.33 sec.) after exposure, as against the total number of exposures, we have the percentage of times

[58] If our guess is correct the fluctuation limen for sounds ought to coincide with the limen found with momentary stimuli. This would be a crucial test and will be made later.

[59] Jackson, *op. cit.*, 602.

[60] Boring, *op. cit.*, 282.

every stimulus-value was perceptible, and from these data we can compute the limens for momentary stimuli, or the limens unaffected by local adaptation. Table I shows how the three limens compare.

Other interesting results, which have been reported before by various investigators but which no one has ever reported or attempted to interpret collectively, have to do with the length

TABLE I
Limens

| *O* | Momentary exposure | Constant exposure | Fluctuation |
|---|---|---|---|
| D | 2379 | 2751 | 3670 |
| H | 882 | 1312 | 1932 |
| G | 1185 | 1357 | 2150 |

of the visibility period, the invisibility period, and the total fluctuation-period as functions of the stimulus-intensity. The curves for D are given in Fig. 5. They show (1) the rate of decrease in the average invisibility period with increased intensity, (2) the decrease in the average visibility period with increased intensity, and (3) the average total period as a function of intensity. It appears that we have here rather simple, continuous functions. Similar curves for the period of invisibility may be found in Wiersma's article[61] for auditory and tactual stimuli as well as visual. We have made curves for the relation between the total period and the stimulus-intensity from Marbe's data[62] and present it in Fig. 6 as a verification of D's curve.[63] All three curves demonstrate the fact that fluctuations are most rapid at a mid-point near the fluctuation limen and decrease in rapidity on both sides. All curves are incomplete, however, so that it is difficult to say to what type of function they belong. They may be parabolic, but a better guess would be that they are inverted normal distribution curves. If that were true, by plotting the frequency of fluctuation, *i.e.* the number of total periods per time-unit, against varied stimulus-intensity we should get a normal probability curve. Further work will be needed to decide the matter.

What does it mean that the fluctuations occur most rapidly at a mid-point and more slowly as we go away from this stimulus-value? It is surely hard to explain on the basis of a rhythm of attention. If we remember that we are dealing with the limen, however, it is more easily accounted for. We have shown that the fluctuations fit nicely into the phi-gamma hypothesis. This means that for any given stimulus there are in the organism a set of factors favoring perceptibility and a set

[61] Wiersma, *op. cit.*, 187 ff.
[62] Marbe, *op. cit.*, 632.
[63] The curves have been smoothed, but they retain their general form.

of factors against perceptibility. The same stimulus, say at the limen, is perceived at one moment because a 'chance' disposition of the factors on the whole favors it; it is not perceived at another moment because the 'chance' disposition is on the whole

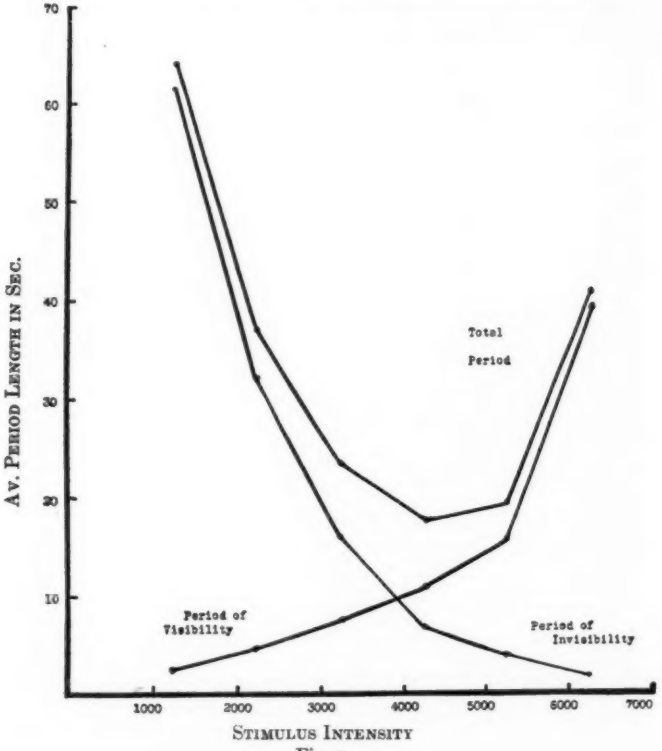



Fig. 5

against it. A fluctuating light or sound would then give us a continuous picture of these 'chance' dispositions, perceptible when the balance of factors is favorable, and imperceptible when the balance of factors is unfavorable. With a stimulus at the limen the factors are balanced so that there is an equal chance of perceptibility or imperceptibility. With stimuli above or below the limen there is a preponderance of one or the other set of factors.

It is conceivable that precisely at the limen, where the balance of factors is best (or worst, as one wishes), a slight change, a shift of but one factor on one side or the other, tips the balance in the opposite direction. Hence changes occur most rap-

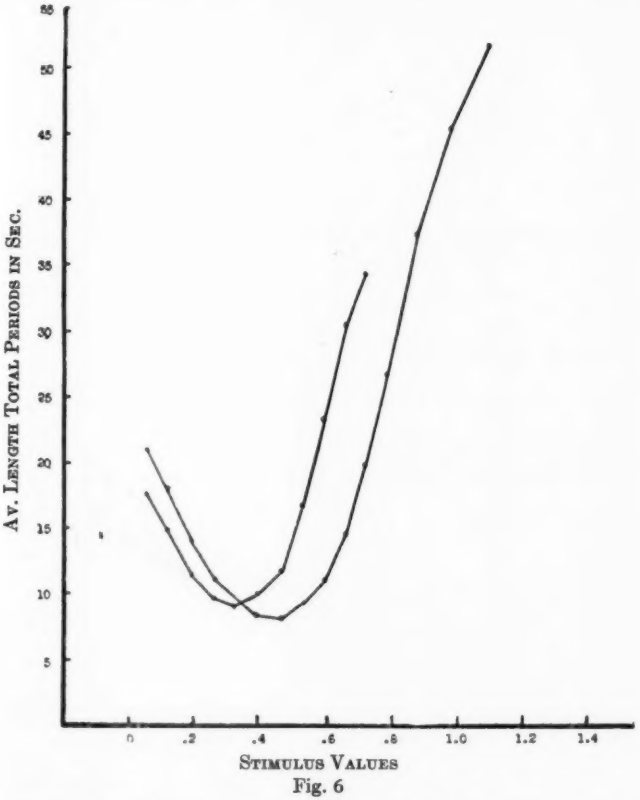



Fig. 6

idly at this point. As we go away from the limen on either side it takes a greater change to turn the balance in the opposite direction. Another suggested interpretation, which probably amounts to the same thing as the first, is that with subliminal stimuli the invisibility period is prolonged by mere inertia; in a similar manner for supraliminal stimuli the period of visibility is prolonged; the thing in mind tends to remain longer than its

corresponding physical intensity will warrant; the thing out of mind has difficulty breaking in.

From the results thus far obtained we may draw the following conclusions. (1) Fluctuations of minimal sensations are a phenomenon of the stimulus limen. (2) The relative amount of the visibility is a direct function of the stimulus-intensity and seems to follow the phi-gamma function. (3) The average period of visibility varies directly with the stimulus-intensity. (4) The average invisibility period varies indirectly as some function of the stimulus-intensity. (5) The fluctuations are most rapid at a mid-point which is near the limen for fluctuations and become less frequent as the stimulus is made more or less intense.

Thus it appears that when we have found the factors which favor fluctuation we shall also have found the factors which control the limen. This allows, of course, for the factor of local adaptation, in the case of vision, which Marbe overlooked. Our next problem will therefore be to test for some of these factors, especially those that are still under debate.

If we view the fluctuations in the above light, the historical results do not seem so conflicting. It is not likely that the fluctuations are due to any one factor, nor that their conditions are exclusively central or peripheral. Indeed, we can find a place for all the acceptable results in such an interpretation as we have offered. Accommodation of the eye undoubtedly presents favorable and unfavorable moments for visibility. Breathing may have an occasional effect upon sensitivity, as many have shown and no one has disputed. Changes in blood-pressure are in doubt as factors at the present moment; but there is no *a priori* reason why they could not produce the occasion for heightened or lowered sensitivity. Local adaptation has been shown to be present with abundant evidence. In order to be effective after the first disappearance, however, there must be some factor which interrupts adaptation and allows for recovery. Eye-movement has been suggested as this factor, with a mass of indirect evidence for it from Ferree.[64] Although Liddell's direct study of this particular problem has led to negative results,[65] we believe that his work should be repeated, taking account of certain technical deficiencies in his work and treating the data more analytically. That is our next task.

## III. Fluctuations and Eye-movements

A repetition of Liddell's experiments seems necessary first of all because many verbal reports from our *O*s express the conviction that eye-movements do facilitate reappearance of the stimulus-light. The following reports are typical:

G. "There were times when I discovered that I brought the stimulus in by dropping my eyes But this occurs only in a small proportion of the cases."

D. "After the initial appearance of the stimulus and its disappearance, it remained out with but one exception in the first experiment and two exceptions in the second. In all of the three exceptions I can report eye movements." "I felt my eyes slip from the fixation-point. I noticed

[64] Ferree, this Journal, 17, 1906, 81 ff.; 19, 1908, 58 ff.

[65] Liddell, *op. cit.*, 252.

PLATE I

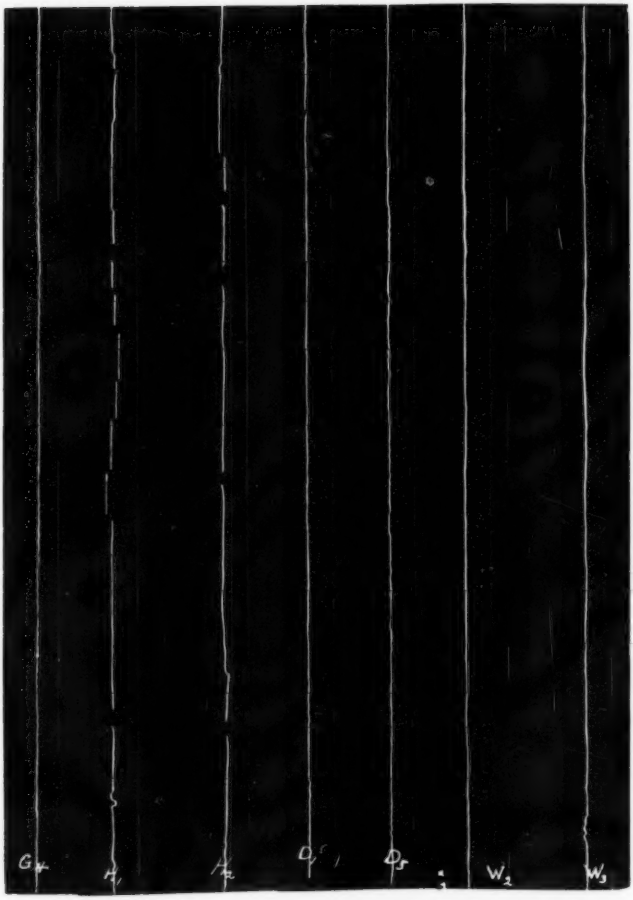



PLATE II

during these slips that the stimulus reappeared." "I also observed that I frequently winked. Following the winks I often noticed that the stimulus came into clear vision." "I think that the upward movements are more efficacious [in bringing back the stimulus-light] than the downward movements. The movements are not symmetrically made; they are more or less dartings-out from the fixation-point and immediate returns to it." "I noticed incipient movements at the appearance and disappearance, usually at the reappearance of the stimulus."

W. "Generally, shortly after its disappearance, I could feel my eyes give a sort of jerk, and as they gave that sort of jerk, the stimulus reappeared."

K. "At times the stimulus would disappear very rapidly and come back immediately. At such times I was aware of a flicker of the eyes as if it moved." "I would feel a sudden movement of my eye and it would suddenly reappear at full intensity."

H. "I noticed that the stimulus staid out for a long time until I would move my eyes. That would bring it back in, then it would suddenly disappear until I moved my eyes again." "Eye-movements played a considerable part. I have three criteria of eye-movement, first the feeling of the eye moving—it gives one a strain and almost a dizzy feeling under these conditions; secondly, the movement of the fixation-point; and thirdly, the sudden popping in (in very clear pattern) of the stimulus-light." "Movement of the eyes does not make it go out, it remains visible, in fact it may become very much clearer. When it is out, however, eye-movements tend to bring it in—with a jerk, suddenly, and in that objective fashion. If the eyes are kept absolutely fixed, I believe that it comes in very gradually, increasing in intensity and clearness."

Liddell also mentions that some of his *O*s were convinced that eye-movements play a rôle in fluctuations. The acceptability of these verbal reports of eye-movement depends upon whether the observation is reliable. When *O* reports an eye-movement, has one occurred or is *O* deceived? Observations which we made during the experiments on eye-movements seem to indicate that *O* scarcely ever reports an eye-movement when none has occurred. We ran a series of experiments in which *O* was to watch fluctuations as usual—with and without a fixation point—but to report with his key the moment he felt an eye-movement. A simultaneous photographic record of these reports with eye-movements (the technique to be described later) was secured.

The records show two distinct types of eye-movements; namely, tremors and re-fixations. Tremors appear in the records as rather small, sharp waves in the eye-line (see Plate I, $W_1$ and Plate II, $D_5$) and are probably tonic movements, some of them obviously made during a pulse-beat and others at random, perhaps a concomitant of rigid fixation or attempts to fixate.[66] Re-fixations appear as sudden breaks in the line (see

[66]Professor Dodge has attributed these tremors to head-movements, chiefly due to the pulse (cf. R. Dodge, An experimental study of visual fixation, *Psychol. Monog.*, 8, 1907, no. 35), and to movements of convergence. It seems that these movements were genuine eye-movements in our cases, for, as we shall see, our *O*s' reports of eye-movements often coincide with them. That they are tonic adjustments of the eyes is suggested by the fact that they often occur immediately after a return of the stimulus-light or its sudden exposure, as may be seen in Plate I $H_3$ and Plate II $D_3$.

Plate II, $H_1$) and are usually greater in extent than the tremors. Table II gives numerical results showing what percent of each of the two types of movement *O* is able to report and, what is more important here, the percent of his reports which were actually preceded immediately by eye-movements. The first row, for every *O*, refers to the eye-movements reported when there was a fixation point, as in the previous experiments on fluctuation, and the second row is for those movements without a fixation-point except for the stimulus itself—a white light 1 cm. sq.—when visible.

TABLE II

Observers' report of eye-movement[67]

| *O* | Tremors | | | Re-fixations | | | Reports | | |
|---|---|---|---|---|---|---|---|---|---|
| | No. | % reported | P.E. | No. | % reported | P.E. | No. | % with movement | P.E. |
| D | 69 | 52.2 | 4.1 | 117 | 1.7 | 0.9 | 40 | 95.0 | 2.4 |
| | 14 | 42.9 | 13.2 | 79 | 12.6 | 2.6 | 16 | 72.7 | 7.5 |
| H | 76 | 34.2 | 3.7 | 55 | 1.8 | 1.3 | 28 | 96.4 | 2.5 |
| | 71 | 54.9 | 4.0 | 50 | 4.0 | 1.9 | 41 | 100.0 | —— |
| G | 29 | 65.5 | 6.0 | 69 | 18.8 | 3.2 | 41 | 71.8 | 4.7 |
| | 90 | 40.0 | 3.5 | 116 | 12.7 | 2.2 | 55 | 92.7 | 2.4 |

It is plain from the table that tremors are more easily detected than re-fixations. On the average, *O* can report half of the tremors which appear in our photographic records, whereas perhaps only 1/10 of the re-fixations are detected. It is important for us here that when *O* reports an eye-movement he may be right over 90% of the time. Let it be added that *O*'s reaction time to these eye-movements was very small, probably of the order of .2 second. Not every eye-movement, therefore, found in the vicinity of his signal was taken as belonging to that signal, but only those which were unmistakably connected with it.

But how are we to account for Liddell's negative results? The records which he published show no significant correlation; nor do our own, when given merely a cursory examination. But an intensive analytical study of our records has justified itself. A statement that "where the eye-movements do occur, they occur at random," without a statistical study to show that they *do* occur at random is not enough. Furthermore, Liddell concludes[68] that "fluctuations of attention are not causally de-

[67]The probable errors were found according to Yule's formula. See H. A. Edgerton, Table of standard errors and probable errors of percentages for varying number of cases, *J. Applied Psychol.*, 10, 1926, 378.

[68]Liddell, *op. cit.*, 252.

pendent upon eye-movements; for some subjects exhibit eye-movements . . . during the invisibility phase without causing the *immediate* recurrence of the visibility phase."[69] Now if an eye-movement is to be held responsible for a return of the visibility phase, we must allow time for two things: (1) the time between the eye-movement and the emergence of the faint light, and (2) the reaction of *O* to the appearance of the light. We happen to know that a part or all of this time is relatively long. As one *O* (D) put it, "It is not so simple as merely reporting the presence or absence of an object. Every report implies a judgment as to whether the stimulus is or is not present; whether it is or is not confused with the retinal lights into which it frequently melts and with which it frequently fuses."

In later experiments, in which the liminal light was suddenly exposed to a new spot on the same retina, the same distance from the fovea, after having disappeared in its original position, *O* did not always see the light immediately. For 4 *O*s the average reaction times to these liminal stimuli were, 0.80 ± .24, 1.04 ± .16, 1.15 ± .60, and 2.37 ± 1.80 sec. Thus if an eye-movement is to have an even chance to provide the favorable occasion for a reappearance, one should allow at least one second, and to be certain that it is not a factor, considerably more time.

*Technique.* The technique of photographing eye-movements follows the Dodge method.[70] The light source for corneal reflection was a small electric arc, and later a 100 w. incandescent lamp with concentrated filament. It was enclosed in a light-proof box which was provided with a round opening 2.5 cm. in diam. over which was placed a condensing lens at a distance of one focal length from the light source. This made an almost parallel bundle of light which was projected out through a tube about 50 cm. long and in diameter a little larger than the opening. Within the tube were placed, at approximately 15 cm. intervals, cardboard washers with openings about 2 cm. in diam. These, with the tube itself, helped to keep the bundle of light parallel. At the outer end of the tube was placed a filter of pot-blue glass plus a minus-red filter to reduce the physiological intensity of the light.

Let us remark here that the presence of this light in the *O*'s visual field is a questionable procedure during fluctuations. It is strong enough to be a constant distractor until after much practice. In the preliminary experiments it was found that the arc light was unsteady. At times some *O*s noted distinct fluctuations in its intensity, for example, W. "The light which was thrown into my eye fluctuated a number of times in violation to the fluctuations of the stimulus. At a number of times both the stimulus and the light were completely obliterated and I got no visual sensation whatever except the dark gray. At other times the light appeared to become dim without completely disappearing. There were a number of fluctuations of the stimulus without any noticeable fluctuations of the light itself, so that the variations were not in a one-to-one correlation, but only at certain times did they appear to be bound together." Such changes—even imperceptible changes—doubtless have a facilitating or inhibiting

[69]Italics mine. [70]Dodge, *op. cit.*

effect upon the stimulus-light according to Heymans' law. We should thus have a new type of fluctuation superposed upon the old. If eye-movements were effective at all in producing the usual fluctuations, their effect would be considerably masked by these new fluctuations. We later changed to the incandescent light source, which, if it physically fluctuated at all, would be accompanied by a corresponding fluctuation of the stimulus-light because they were on the same local electric circuit. We thus had eye-movement records from both kinds of light source. Both apparently correlate equally well with fluctuations, although the number of records from the arc light are too few for us to be sure on this point. The disadvantage of the incandescent light is that it is not so nearly a point-source and gives a less sharp eye-line on the film than does the arc-light.

Besides the unsteadiness of the blue light, found in W's report, in the case of the arc, there is the second disturbing tendency for the whole field to 'go blind,' as D reports it: "at times it seems as if the whole field of vision goes blind; that is, it seems as if nothing is being seen with the left eye." Attempts were made entirely to eliminate any visible photographic light by the use of filters which transmit only ultraviolet wave-lengths. With the combination of an ultraviolet filter and a minus-red gelatin all visible rays were eliminated, but the intensity was so weak photographically as to make no impression on the film. With the ultraviolet filter alone the visibility was reduced to a negligible minimum; but no readable impression could be obtained upon the film. It may be that one could succeed by using quartz lenses throughout in place of glass, but we did not try this procedure. In the face of all these difficulties, therefore, any correlation found between fluctuations and eye-movements will be very significant.

The beam of blue light was directed from the rear and to the left of the *O* into a mirror which was directly below the camera lens. The latter was about 18 cm. from the cornea of *O*'s left eye. The mirror was so adjusted that the reflected beam fell upon *O*'s left eye completely covering the cornea, where a tiny bright spot was produced. It is this brilliant spot that is photographed in the Dodge method.

The camera was 55 inches long from the lens to the film. In place of Dodge's falling plate device a panoramic camera was attached for carrying the film. The film, which was 5 feet by 6 inches, was wound up on a drum at the rate of 16 mm. per sec., the slowest speed attainable with the spring-motor of the panoramic camera. The film was always run in the vertical direction so that horizontal eye-movements, which are the most numerous, show in the records as breaks or waves in the eye-line. Extremely few movements are exclusively vertical, so that practically all movements will show in the records.

In order to get a simultaneous record of fluctuations beside the eye-line we made use of a small flash-light in circuit with *O*'s reaction-key. This signal light was furnished by a 3.5 v. concentrated filament bulb mounted in the end of a brass tube of variable length. In the other end of the tube, about 20 cm. distant, was a short-focus lens for producing an image of the light on the film. The tube was mounted inside the camera-box to one side of the path of light from the eye, with its lens about 22 cm. from the film. When *O* pressed his key to indicate a disappearance the light traced a straight line on the film beside the eye-line.

In order to insure that the two images were even with each other, they were brought together upon a focusing screen before each series of fluctuations. *O*'s head was held fast by a very rigid biting-board and head-rest. During the series *E* could see the two images from the back of the film. If they should not stay abreast the film could be stopped momentarily and the two dots produced at this point from over-exposure indicated their relative positions.

*Procedure.* *O*'s right eye was covered throughout the experiments to avoid any stimulation and subsequent retinal rivalry. In every experimental hour *O* adjusted his position in the head-rest and biting-board and the camera was focused. In preliminary trials *O* made several adjustments of the stimulus-light in order to find an intensity which would fluctuate quite freely. Once this value was established it was used throughout all later experiments. The stimulus was a white patch of light from the Nagel adaptometer 1 cm. square, to agree with that used by Liddell, without a fixation point. *O* was accustomed to the sound of the camera during preliminary trials. This sound was constant and scarcely noticed by *O*. As we shall show later, distracting stimuli—other than visual—during fluctuations are no more serious than in the average psychological experiment. Each series lasted 97 sec. Before every one the camera was readjusted so as to get the eye spot and signal light together and in focus. As soon as *O* began reporting fluctuations the camera was started. After every series *O* had a short rest during which he gave verbal reports. Six series as a rule were secured in each experimental hour. We have on the whole at least 18 min. of fluctuations and eye-movement records from every one of the 4 *O*s, using the Liddell conditions as nearly as we could reproduce them.

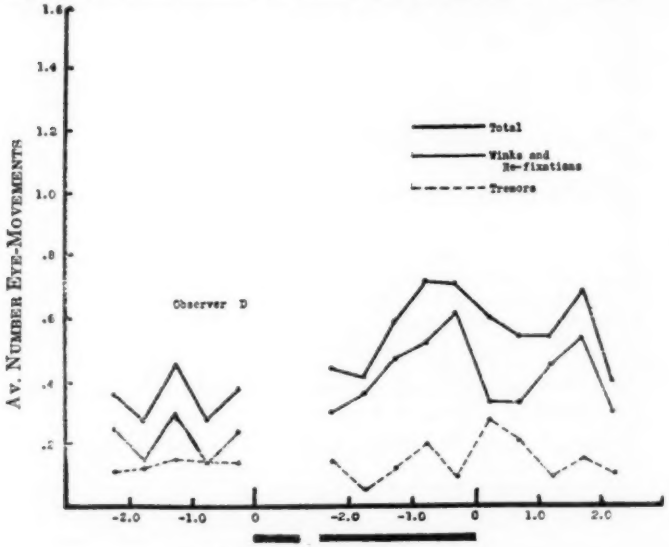
*Results.* The special questions which we hoped to answer from the eye-movement records were the following. (1) Are there more eye-movements in the interval just before a reappearance than at other times during fluctuations? (2) Do eye-movements during the invisibility phase increase in frequency up to the point of reappearance? (3) Is the eye more or less quiet just before a disappearance? (4) Is there a direct correlation between the number of fluctuations and the number of eye-movements during a single series?

In answer to the first three questions we have counted the number of eye-movements during three representative phases: the 2.5 sec. just before a disappearance; the same time just before and just after a reappearance.[71] The periods were divided into ½ sec. intervals in order to note any trends in frequency of eye-movements during each phase. We chose 2.5 sec. as the limit because eye-movements during that time just preceding disappearance or reappearance may have some influence upon those events. The time was not made greater than 2.5 sec. because the average periods of visibility and invisibility are of course relatively short. Many invisibility periods are shorter than 2.5 sec.; none shorter than 1 sec. was included in the count. Visibility periods were not included in the count if less than 2.5 sec.; even in those less than 5.0 sec. in length the periods just after one reappearance overlap those just before the next disappearance. Fortunately the visibility periods were relatively longer than the invisibility periods, as is almost invariably the case when *O* is allowed to select his own stimulus; he selects one above his limen of fluctuation.

We counted separately and collectively, three kinds of movements, tremors, re-fixations, and winks. Winks were included because the records show that the eye makes a lateral movement at the same time. We make no claim of accuracy in counting the number of tremors, but every perceptible one—a movement out and back—in the eye-line was counted, depending upon the sharpness of focus of the eye-line and the depth of impression on the film. The re-fixations and winks are usually unmistakable in the records.

[71] We are indebted to Dr. E. Culler for suggestions on the treatment of the data on eye-movements.

The curves in Figs. 7, 8, 9, and 10, represent in clearest form the results. The average number of movements is based upon 30-80 cases of disappearance or reappearance. The curves may be interpreted as follows: (1) There is usually a minimum of activity of the eye in the 2.5 sec. interval before a disappearance, especially in the number of winks and re-fixations. Further-




Phase of Invisibility
Fig. 7

more, the total number of movements per ½-sec, tends to become progressively smaller as the moment of disappearance is approached. This no doubt furnished more favorable conditions for local adaptation. (2) The phase just before a reappearance shows the greatest amount of oculomotor activity, especially in the number of winks and re-fixations. (3) Of all the ½-sec. intervals studied, the second one before a reappearance shows the greatest number of winks and re-fixations, and the first and third intervals usually come next.

There is a second high point in the curve about 2 sec. after reappearance for which we are unable to find any significance. This hyperactivity apparently subsides before the next disappearance. It may be a continuation of the marked instability of the eye exhibited just after the reappearance. This instabil-

ity is extreme in the case of 2 *O*s (H and Ha), but shows itself mostly in the form of tremors. It is due to the necessary readjustments which the eye makes to the returned stimulus-light. Fixation is partially or entirely lost during the invisibility phase due to wandering or 'searching' movements when there is no fixation-point. Even where fixation is more stable,

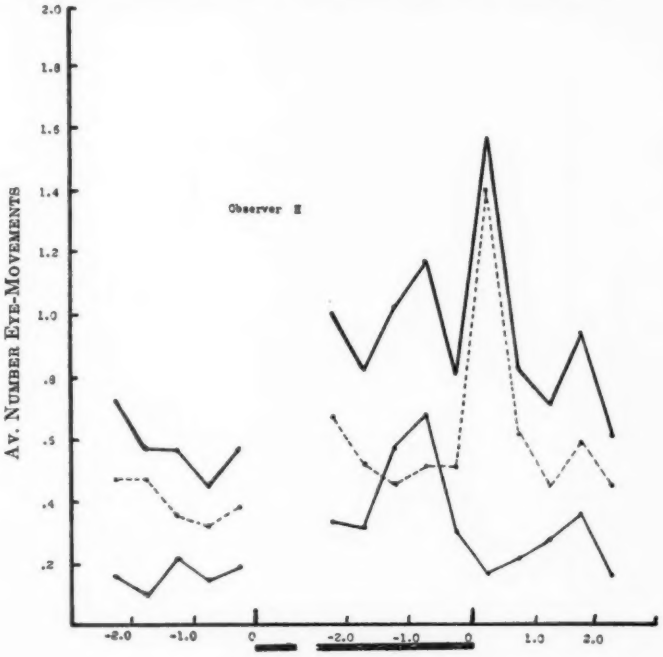



Fig. 8

as in the case where there is a fixation-point, the reappearance of the additional light in the field apparently has its motor consequences in the eye. (See Plate I, $H_3$; and Plate II, $D_5$). The fact first mentioned, that the most frequent period for winks and refixations comes during the 1.5-sec. interval just before the return of the stimulus-light, is all the more significant when we recall that this coincides with the reaction times of the observers to these liminal lights.

Examples of good correlations between eye-movements and fluctuations will be found in Plate I, $G_1$, $G_2$, $D_3$, $D_4$, and $H_3$. The records are to be read from the top down. The straight white lines represent invisibility periods.

On the negative side, where eye-movements are rare or entirely eliminated, fluctuations should decrease in frequency or cease altogether. There is some evidence of this sort in one case.

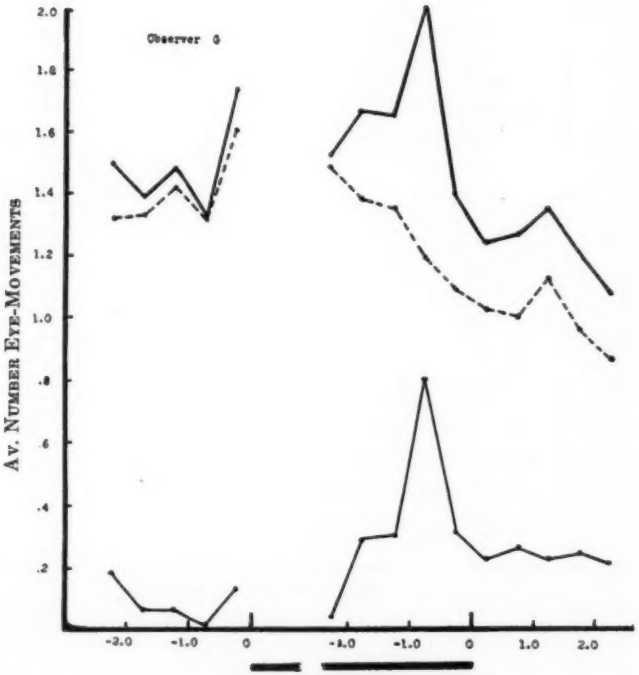



Fig. 9

D had, in the previous experiments, a number of series of fluctuations in which the stimulus-light would disappear and remain out for the rest of the period of 3 min. Coupled with such a series were verbal reports of which the following is typical. "During the experiment I made an extraordinary effort to keep fixation constant. I can report that the eyes did not slip from the fixation-point a single time. . . . At no time, as the signals during the experiment show, did the stimulus appear." For-

tunately a similar series occurred during the photographing of eye-movements so that we are able to test D's report of good fixation. Plate I, $D_2$ shows a part of the record. There was a fixation-point, as in similar earlier series, and the stimulus was exposed in parafoveal vision. There are a few slight re-fixations, but no distinct tremors. It is interesting to compare this record with the next two—$W_1$ and $G_1$—in which there are no re-fixations, but numerous tremors. In both these cases there were fluctuations.

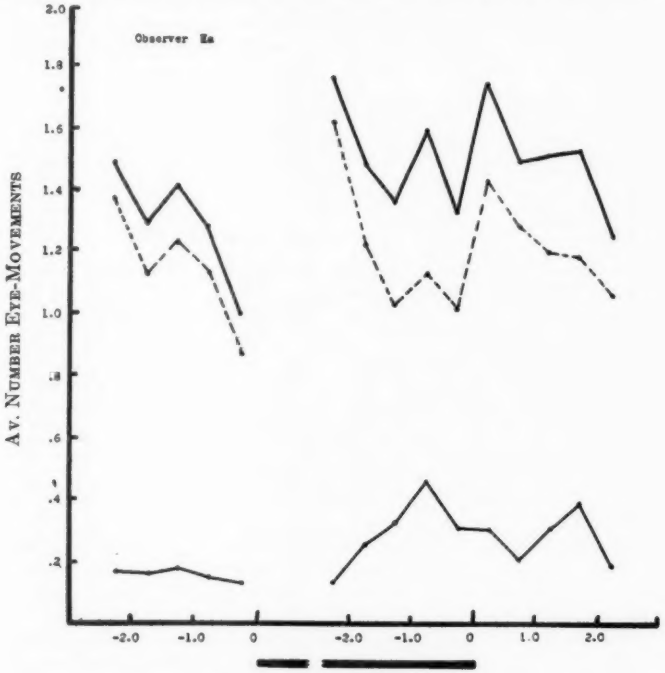



Fig. 10

Whether or not the comparison is significant we are unable to say, but it may mean that the presence of tremors favors fluctuations, while the absence of them removes a sufficient condition for the return of the stimulus-light, if not a necessary condition.

Another fact which was noted during the counting of eye-movements was the frequent coincidence of winks or re-fixations and brief disappearances, nearly always less than 1 sec. duration. We therefore studied all cases of disappearance of less than 1 sec. to see in how many of these cases a wink or re-fixa-

tion actually preceded. Examples of this coincidence may be found in Plate I, $D_4$ and $H_2$. Table III gives the results.

In the case of 2 *O*s at least half of the momentary disappearances were preceded by a wink or re-fixation. This is significant only when we examine the curves again, and find that the usual disappearances—those larger than 1 sec.—are unusually free from disturbances of this kind just preceding. What happens in the case of these short disappearances is probably this: during the wink or shift of fixation accommodation lapses, or else,

TABLE III
Momentary disappearances preceded by eye-movements

| *O* | Number | Percent preceded by | | | |
|---|---|---|---|---|---|
| | | Winks | Re-fixations | Either W or R | P.E. |
| D | 24 | 29.1 | 29.1 | 58.2 | 6.7 |
| H | 21 | 28.5 | 28.5 | 57.0 | 7.2 |
| G | 17 | 0 | 47.1 | 47.1 | 8.1 |
| Ha | 13 | 15.3 | 7.7 | 23.0 | 8.5 |

as is generally known, the eye mediates no clear image due to a moving physiological stimulus. The stimulus-light is thus momentarily lost. When the eye comes to rest again a new image is mediated and *O* reports a reappearance. That these disappearances are so brief is also significant. Ordinarily a stimulus-light so weak that a chance disturbance causes its complete loss remains invisible for the usual period. Apparently where a wink or a re-fixation furnishes the disturbing factor, at the same time and in other ways, it promotes favorable conditions for visibility the moment the eye comes to rest.

Our fourth special question, whether there are more fluctuations in the series where there are more eye-movements, cannot be answered from our data, nor is an answer so necessary after finding the more direct correlations. The rapidity of fluctuation is determined by many factors and in general varies with the distance of the stimulus from the limen. We did not establish a limen for the stimuli used during this photographic work, and the number of single series is so small that it would be useless to attempt a correlation between frequency of eye-movement and of fluctuations. We have only a rough indication of such a correlation in series like those in Plate I, $D_2$, $W_1$ and $G_1$. There is a decided absence of tremors in the first and an abundance of them in the second and third, whereas there are a few re-fixations in the first and none at all in the other two. If this correlation between frequency of tremor and fluctuations holds, we would have to conclude that whereas re-fixations furnish an occasional condition for visibility, tremors are a constant factor for visibility. Further evidence will have to be found before such a conclusion is justifiable.

Our data, like those of Liddell's, show many instances where fluctuations seem to go on independently of eye-movements. On the one hand, there are numerous reappearances without a detectable eye-movement other than the regular tremors. (See Plate I, $W_1$ and Plate II, $G_4$.) The possibility that a mere innervation of the eye-muscles is sufficient to reinforce the weak light, as McDougall has suggested, has not been eliminated. Such innervation might be expected to produce tremors in the non-atropinised eyes of our *O*s and these tremors should appear in the records. But there is little evidence from our curves that even the noticeable tremors have any occasional effect of this kind, of immediate reinforcement of the weak light. On the other hand, there are long invisibility periods in which numerous re-fixations seem to have little effect in bringing back the light. (See Plate II, $H_1$ and $H_2$.) These two instances, along with the incomplete correlation we find between eye-movement and fluctuations, furnish additional evidence that there are other factors apart from the oculomotor apparatus which make for or against perceptibility of a liminal stimulus.

From this section of our general problem we have evidence for the following conclusions. (1) Local retinal adaptation is a factor in the disappearance of lights during fluctuations with liminal stimuli. (2) Winks and re-fixations are occasional factors favoring the return of a liminal light which has disappeared. (3) Eye-tremors may furnish a constant factor for visibility. (4) Winks and re-fixations often bring about momentary disappearances of a liminal light. (5) There are other factors than eye-movements which promote the return of a liminal light which has disappeared.

We shall now turn to the central part of the optic sector to look for other factors.

## IV. Tests for Central Factors

If we take our historical background alone, we shall have little except theory to begin with. We have fatigue theories on the one hand and theories of facilitation and inhibition on the other, or hypothetical combinations of these effects. Without going into a detailed exposition, let us see what the theories can be made to mean in general terms. There may be a general cortical fatigue, as first suggested by Lange in connection with fluctuations,[72] or a local fatigue according to McDougall's physiological theory of attention.[73] There are also the 'facilitation' and 'inhibition' theories of Lehmann[74] and Slaughter[75] to account for the connection between breathing and the

[72]Lange, *op. cit.*, 420. [73]McDougall, *op. cit.*, 349 ff.
[74]Lehmann, *op. cit.*, 84 ff. [75]Slaughter, *op. cit.*, 332 ff.

Traube-Hering waves and fluctuations. In this part of our problem we have confined ourselves to a study of the fatigue theories. We have now to ask whether the excitation of a part of the optic sector produces a condition in the central portion which reduces the receptivity for the same excitation.

This question may be answered by relying upon the fact that for corresponding points in the two retinas, the central portions of the optic sector are anatomically, and perhaps also functionally, identical. More specifically stated, we can present a liminal stimulus to a certain area of one retina, then, when it has disappeared, transfer the same stimulus to the corresponding area of the other eye. If there is a central fatigue and if this were the only factor, the light should remain invisible for the usual period. If, on the other hand, the conditions are entirely peripheral, the light should reappear immediately.[76]

This test was applied in the earlier part of our work with ambiguous results. The technique, to be described later, was so arranged that the moment *O* pressed his key to denote a disappearance the stimulus was automatically shifted to the other eye. Since *O* was working in a completely dark room this shift could be accomplished without his knowledge, and he could not always tell which eye was receiving the stimulation. One *O*, W, nearly always reported an immediate return of the light under these conditions; another *O*, Ho, reported normal fluctuations as if the stimulus had been continuously exposed to one eye; a third *O*, Di, gave mixed reports; at times the light came back immediately and on rarer occasions it did not. It was obvious that the conditions would have to be better controlled. We did not know whether the stimuli used were above or below the limen. Probably they were above the limen, since the *O* in every case selected the stimulus which would fluctuate. We did not know whether the stimulus was always striking corresponding points in the two eyes. In some cases it obviously was not, for *O* often reported that it moved slightly in pressing down his key. Since there was some evidence for central fatigue here we proceeded to find the limens for the *O*s in the later experiments and to provide a fixation point.

Assuming that there is some fatigue in the central part of the optic sector, we have the additional problem of its extent. Does it spread beyond the parts immediately involved in this particular neurological pattern? Is the spread, if any, limited to one hemisphere? These new questions may be tested by shifting the stimulus from one eye to another on non-corresponding points, involving in the center one hemisphere or both. This procedure can be checked by a shift from one area to another in the *same* eye, also involving one hemisphere or both. We have therefore 6 variations which will hereafter be known as follows:

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[76]We are indebted to Professor Kurt Koffka for suggesting this test of central *vs.* peripheral conditions, and to Miss Ruth Burke for aid in the statistical treatment of data in this section.

IA, stimulus continuously presented to the same area, one eye.

IB, stimulus shifted, after disappearance, to the corresponding area of the other eye.

IIA, stimulus shifted to a new area of the same eye, same hemisphere involved.

IIB, stimulus shifted to a new area of the same eye, the other hemisphere involved.

IIIA, stimulus shifted to the other eye, non-corresponding points, same hemisphere involved.

IIIB, stimulus shifted to the other eye, non-corresponding points, the other hemisphere involved.

As control experiments, an arrangement was so made that the stimulus was not shifted at the moment of disappearance, but at varied intervals afterward. The stimulus thus continued to act upon the original area longer than for complete 'adaptation,' and its effects upon the sensitivity for the new stimulation should therefore be intensified. This control will be designated as $C_1$ and it will be found in connection with all the variations except IA. A second control, to be called $C_2$, consisted in delaying the transfer of stimulation but in cutting the stimulation off from the original area in the interim. This interval of rest should allow for some degree of recovery from the effects of the original stimulation and the results should present a contrast to those in $C_1$ if there are transferred fatigue effects from the first stimulation to the second. It also gave us a basis for establishing the normal reaction-times to the liminal stimuli which we used in the previous section.

*Technique and Procedure.* The stimulus was the same as in Section II of this paper, a white circular patch with a red fixation-point. The IB series were alternated with the IA series from which the data in Section II are taken, so that *O* rarely knew whether the stimulus was given to one eye or alternately or to both.

The method of transferring the stimulus from one eye to the other was accomplished by means of a cardboard shutter 17.5 x 4.2 cm. In IB the shutter was placed vertically before *O*'s face about 35 cm. distant, so that the fixation point was visible to both eyes just above the upper end. When the shutter was turned slightly to *O*'s right, the stimulus was exposed to his left eye but not to his right. The shutter was fastened at its lower end to a rotary solenoid, the electromagnet and core from an automatic piano player. A current sent through the magnet coils turned the shutter to the left; a spring turned it to the right. The solenoid was controlled through a secondary electric switch in circuit with *O*'s key, so that when *O*'s key was pressed to denote a disappearance, the shutter would move from left to right, or from right to left, according as *E* desired and set the electric switch. In arrangement $C_1$ the shutter was controled by *E*'s key.

The shutter was so placed, and so adjusted in width, that the stimulus would begin its exposure to the second eye the moment the cutting-off began for the first eye, so that the complete circle was exposed to one eye or to the two jointly without intermission or duplication if possible. The

shift was instantaneous. Although its reaction-time was not determined, it must have been of the order of 100 sigma, or less, since, in some of the other arrangements in which the stimulus was exposed in two different positions, phenomenal movement occurred. Since the stimulus was directly below the fixation-point and in the same plane, it should strike corresponding areas in the two eyes. Since convergence is often inaccurate in the dark, however, the stimulus might appear double for the two eyes. In case *O* reported that it appeared in two slightly different positions, the apparent distance of the fixation-point could be altered in the mirror until the stimulus appeared in identical positions for the two eyes.

In IIA two similar circular patches 42 mm. in diam. were exposed to the left of the fixation-point, one above the other, both about 40 mm. from this point. The stimuli were shielded from the right eye and the shutter was now placed nearer to the stimuli and in the horizontal position and adjusted so as to cover the two alternately. The fixation-point was visible to both eyes.

In IIB two similar circles were placed below and to the right and left of the fixation-point, again 40 mm. distant. The right eye was shielded from the two but not from the fixation-point. The shutter was placed in the vertical position as in IB but nearer to the stimuli so as to cover them alternately.

IIIA is a more difficult procedure. Both stimuli must be presented on the same side of the fovea, one to the right eye and one to the left. The arrangement of stimuli and shutter was as in IIA, but a cardboard shield, placed close to the eyes, was so cut off that the right eye was exposed to the lower spot when the shutter uncovered it and the left eye was exposed to the upper spot when the shutter uncovered it, and both eyes were exposed to the fixation-point at all times.

An entirely different but fairly simple procedure was followed in IIIB. Here the fixation-point was brought apparently much nearer than the plane of the stimulus. The single stimulus spot was therefore seen in two positions for the two eyes and the required shift was produced, *i.e.* from one eye to the other, involving non-corresponding retinal points and two hemispheres. The shutter was placed in the vertical position and merely shut off the view alternately for the two eyes as in IB.

In the control experiments ($C_2$), a new shutter was introduced similar in every respect to the first but 7.0 by 17.5 cm. It was placed nearer to the stimulus than the first and was always controlled by *E*'s key, covering or uncovering a stimulus-spot for either eye or both as desired when the key was pressed. The usual procedure in $C_2$ was as follows. *E* would close his key so that the stimulus next to be exposed to *O*, when he closed his key to denote a disappearance of the first stimulus, was covered. *O* would press his key to report a disappearance of the original stimulus and from 1–6 sec. later *E* would release his key, exposing the new stimulus. Both keys were in circuit with writing points which made graphic records along with a time-record.

The amount of time devoted to each variation differed from one *O* to another. At least two days were given to every one of the 3 or 4 chosen stimuli in IB. These series were alternated with the IA series and with control series. For the other variations usually only the liminal stimulus was used, two days for each variation with control series included.

*Results.* The evidence for or against central fatigue comes by way of a comparison of the results from IA and IB. As in the preliminary experiments we find individual differences, although not nearly so great. Three *O*s showed a mixed form of reaction in IB; sometimes the stimulus-light would be seen

immediately as soon as it was shifted, at other times it would not. Ha consistently reported an immediate reappearance.

Variations are extreme in both IA and IB as one might expect where there is a complexity of conditions and none of them under rigid control. We feel justified therefore in all the following numerical comparisons in basing our conclusions upon averages and the differences between averages, depending upon a multiplication of cases to weight the evidence in one direction or another. The data which were secured during the 10–15

TABLE IV[77]

| O | Stimulus Value | IA | | | IB | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2250 | 22 | 32.09 | 4.900 | 89 | 2.06 | .120 | 30.09 | 4.901 | 6.14 | 1.000 |
| | 2750 | 24 | 17.17 | 2.831 | 88 | 1.71 | .071 | 15.46 | 2.832 | 5.46 | 1.000 |
| | 3250 | 48 | 15.77 | 1.854 | 110 | 1.52 | .074 | 14.25 | 1.854 | 7.69 | 1.000 |
| | 3750 | 15 | 13.56 | 3.713 | 33 | 3.08 | .280 | 10.48 | 3.723 | 2.81 | .971 |
| H | 800 | 36 | 10.46 | 1.773 | 83 | 5.17 | .404 | 5.29 | 1.818 | 2.91 | .975 |
| | 1150 | 93 | 7.81 | .554 | 174 | 3.56 | .282 | 4.25 | .622 | 6.83 | 1.000 |
| | 1500 | 129 | 5.78 | .354 | 180 | 2.35 | .143 | 3.43 | .382 | 8.98 | 1.000 |
| | 1900 | 128 | 5.66 | .375 | 157 | 2.69 | .203 | 2.97 | .426 | 6.97 | 1.000 |
| G | 925 | 36 | 19.47 | 2.355 | 38 | 14.47 | 1.657 | 5.00 | 2.879 | 1.74 | .879 |
| | 1200 | 35 | 12.74 | 1.569 | 77 | 7.07 | .908 | 5.67 | 1.813 | 3.13 | .982 |
| | 1475 | 75 | 11.95 | 1.035 | 109 | 6.95 | .624 | 5.00 | 1.208 | 4.14 | .997 |
| | 1750 | 64 | 10.36 | .880 | 32 | 6.41 | .836 | 3.95 | 1.214 | 3.25 | .986 |
| Ha | All | 338 | 4.03 | .141 | 444 | .85 | .024 | 3.18 | .142 | 22.39 | 1.000 |

min. interval after the beginning of dark adaptation were not used here because the retina had not yet reached a stage of equilibrium. Series were also excluded in which an *O* reported mists and hazes to be so numerous as to interfere with observation.

When we compare the averages for the invisibility periods under conditions IA and IB, we find a decided difference in favor of the former. The data in Table IV show beyond doubt that the averages of invisibility periods for IA will almost certainly be greater than those for IB under our conditions. In the case of G only does there seem to be a chance for an average to be greater for IB. This result might mean that the central factors, if they exist at all, have a minor influence upon fluctuations. But the test is not a fair one. Although identical projection areas may be involved in the two eyes, a change of eyes changes a considerable portion of the whole retino-cortical sector. A change of a part of the optic sector may be equivalent to changing the whole as the configurationists so often tell us. Besides, many single invisibility periods in IB are so prolonged

[77]In most of the tables it will be noticed that all the results for Ha are averaged together. This was done because a change of stimulus-intensity, within the rage of stimuli used for her at least, made no difference in the results.

that there appears to be an inhibition of some sort, as indicated also in some verbal reports. D reported on one occasion, "Sometimes the stimulus immediately reappeared and at other times it continued to stay out." He remarked, "It stays in for a long time and disappears and stays out for a long time.. . . Sometimes when the light gray disappeared it would reappear very quickly and fairly brightly, but at other times there was the long duration I have mentioned before."

This delay in return is not necessarily due to the mists and hazes which sometimes hide the stimulus-light. The delay occurred in series after which *O* reported that the field was exceptionally free from mists or hazes, and in *E*'s own observation the field during these delayed returns often takes on an exceptionally black uniform appearance.

TABLE V

| *O* | Stimulus Value | $IBC_1$ | | | $IBC_2$ | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2250 | 15 | 1.96 | .155 | 14 | 1.69 | .126 | .27 | .173 | 1.56 | .853 |
| | 2750 | 14 | 1.91 | .147 | 26 | .97 | .012 | .94 | .147 | 6.39 | 1.000 |
| H | 1500 | 18 | 1.12 | .028 | 19 | 1.22 | .119 | −.10 | .122 | .82 | .710 |
| | 1900 | 17 | .95 | .017 | 15 | .62 | .018 | .33 | .025 | 13.20 | 1.000 |
| G | 1750 | 24 | 2.80 | .563 | 18 | 1.43 | .219 | 1.37 | .604 | 2.27 | .937 |
| Hs | All | 22 | 1.03 | .098 | 64 | .74 | .025 | .29 | .101 | 2.87 | .973 |

We cannot infer from the first comparison alone whether there has been any lengthening of the average period at all due to central factors. A comparison of $IBC_1$ and $IBC_2$ will give some indication of this because, in the former arrangement, $IBC_1$, the stimulus is left upon the first eye after disappearance to continue its effects; whereas in the latter, $IBC_2$ the stimulation is cut off the first eye and not for some time exposed to the second. If, then, there is an inhibitory effect of the first stimulation upon the second the invisibility periods for $IBC_2$ should be shorter than those for $IBC_1$. This is, in fact, the case as demonstrated in Table V. There is an exception in only one *O*, H, where with one stimulus-value the difference is negative, but with uncertain probability.

There is another test which consists in a comparison of the averages from IB with those from IIA. In the latter the stimulus is shifted to a new area of the same eye, thus involving the same eye but different retinal and cortical elements. In Table VI the comparison shows that in every case the averages from IB are longer than those from IIA and we may be almost certain that they will always be so under our conditions.

But it may be objected that in IIA new factors come in. One is the fact that the stimulus reappears in a new phenomenal direction and another is that there is phenomenal movement. When *O* presses his key to denote a disappearance and the stim-

TABLE VI

| *O* | Stimulus Value | IB | | | IIA | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N. | Av. | P.E.$_{Av.}$ | | | | |
| D | 2250 | 89 | 2.06 | .120 | 42 | 1.76 | .139 | .30 | .183 | 1.64 | .865 |
| | 2750 | 88 | 1.71 | .071 | 122 | 1.15 | .054 | .56 | .089 | 6.29 | 1.000 |
| | 3250 | 110 | 1.52 | .074 | 55 | .75 | .032 | .77 | .081 | 9.51 | 1.000 |
| H | 1150 | 174 | 3.56 | .282 | 30 | 1.11 | .060 | 2.45 | .288 | 8.51 | 1.000 |
| | 1500 | 180 | 2.35 | .143 | 25 | 1.04 | .032 | 1.31 | .146 | 8.97 | 1.000 |
| | 1900 | 157 | 2.69 | .203 | 34 | .91 | .027 | 1.78 | .204 | 8.73 | 1.000 |
| G | 1475 | 109 | 6.95 | .624 | 117 | 3.05 | .218 | 3.90 | .661 | 5.90 | 1.000 |
| | 1750 | 32 | 6.41 | .836 | 33 | 3.23 | .507 | 3.18 | .977 | 3.25 | .986 |
| Ha | All | 444 | .85 | .018 | 131 | .80 | .021 | .05 | .028 | 1.79 | .886 |

ulus is automatically shifted to a new position above or below, as the case may be, he invariably sees movement, which is described in general as follows:

Ha. "The typical proceeding was, brightness appeared at the bottom [as *O* pressed her key to denote a disappearance at the top], flash off [as *O* released her key to denote a reappearance, the shutter covered it], then a dimmer brightness at the top, dying out gradually and leaving behind a black hole [negative after-image]." Again, "Below there appeared a rather good brightness and this progressed toward the top. There was more progression or movement here." D reported: "The stimulus was in the lower position. It disappeared, and as I signaled it reappeared above; when I signaled that I saw the light it immediately fell down to the original position and was visible during the whole drop. When it reaches the bottom the phenomenon is repeated." Again: "The movement that the stimulus makes when it jumps above attracts attention and, even though the retinal lights are strong, differentiates it from the retinal light."[78]

The objections are minimised if we use instead of IIA, the control series IIA$C_2$. Here the second stimulus is not uncovered until 1–6 sec. after *O* has signaled the disappearance of the first. No movement occurs and although *O* learns to expect the reappearance in a certain position, the arrangement is almost equivalent to the presentation of an entirely new stimulus. Indeed, it is chiefly upon these reactions that we have based our reaction-times to liminal stimuli. Table VII shows that

[78]This case of phenomenal movement is interesting as a by-product of the study. The time-interval for shifting stimuli is evidently of the order of magnitude which produces phenomenal movement. The movement here is opposed in direction to that of the moving shutter and even opposed to the order of exposure of parts of the stimulus-object; *i.e.* the remotest parts of the second stimulus are exposed first and the nearest parts last and the first stimulus is cut off in the reverse order. Facts which verify Wertheimer's cortical theory were quite evident in arrangements IIB and IIIB in which the two stimuli are given to the two eyes separately and good phenomenal movement occurred.

there is still a delay in the reappearance in the case of IB. The differences in averages are practically certain for 3 *O*s but lie in both directions with much less probability in either direction for D.

TABLE VII

| *O* | Stimulus Value | IB | | | $IIAC_2$ | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2250 | 89 | 2.06 | .120 | 19 | 2.50 | .390 | −.44 | .408 | 1.08 | .771 |
| | 2750 | 88 | 1.71 | .071 | 18 | 1.65 | .231 | .06 | .242 | .25 | .567 |
| | 3250 | 110 | 1.52 | .074 | 22 | 2.04 | .360 | −.52 | .367 | 1.42 | .831 |
| H | 1150 | 93 | 3.56 | .282 | 26 | 1.12 | .027 | 2.44 | .283 | 8.62 | 1.000 |
| | 1500 | 180 | 2.35 | .143 | 13 | 1.03 | .105 | 1.32 | .177 | 7.46 | 1.000 |
| | 1900 | 157 | 2.69 | .203 | 21 | .76 | .035 | 1.93 | .206 | 9.37 | 1.000 |
| G | 1475 | 109 | 6.95 | .624 | 9 | 2.69 | .520 | 4.26 | .812 | 5.25 | 1.000 |
| | 1750 | 32 | 6.41 | .836 | 39 | 2.73 | .261 | 3.68 | .870 | 4.23 | .998 |
| Ha | All | 444 | .85 | .018 | 89 | .75 | .019 | .10 | .026 | 3.85 | .995 |

Another objection may still be offered. Do we know that the sensitivity of the two chosen areas is the same? To be sure, they are the same distance from the fovea, but one is below the fovea and the other is to one side of it. We can determine this, possibly, from a comparison of the results from $IIAC_2$ with those of $IBC_2$. $IBC_2$ should give us the approximate reaction-times and hence should indicate the sensitivity of the area used in IB as compared with that used in IIA and $IIAC_2$. If the averages for $IBC_2$ are less than, or equal to, those for $IIAC_2$, then the area used in the former is just as sensitive or more sensitive than that used in $IIAC_2$. This is apparently the case, as revealed in Table VIII. The differences lie in both directions and, if favored in either direction, they would probably lie on the side of $IIAC_2$ as being greater. The only other obvious difference which may exist in the two cases is that IB involves two

TABLE VIII

| *O* | Stimulus Value | $IBC_2$ | | | $IIAC_2$ | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2250 | 14 | 1.69 | .126 | 19 | 2.50 | .390 | −.81 | .410 | 1.98 | .909 |
| | 2750 | 26 | .97 | .012 | 18 | 1.65 | .231 | −.68 | .231 | 2.94 | .976 |
| | 3250 | 20 | .85 | .031 | 22 | 2.04 | .360 | −1.19 | .361 | 3.29 | .986 |
| H | 1150 | 4 | 1.97 | .400 | 26 | 1.12 | .027 | .85 | .401 | 2.12 | .923 |
| | 1500 | 19 | 1.22 | .119 | 13 | 1.03 | .105 | .19 | .158 | 1.20 | .791 |
| | 1900 | 15 | .62 | .018 | 21 | .76 | .035 | −.16 | .039 | 4.10 | .997 |
| G | 1475 | 4 | 4.47 | 1.405 | 9 | 2.69 | .520 | 1.78 | 1.497 | 1.19 | .789 |
| | 1750 | 18 | 1.43 | .219 | 39 | 2.73 | .261 | −1.30 | .347 | 3.75 | .993 |
| Ha | All | 64 | .74 | .025 | 89 | .75 | .019 | −.01 | .031 | .32 | .585 |

eyes and two hemispheres whereas IIA involves one eye and one hemisphere. But IIIB also involves both eyes and both hemispheres and the data show that its average periods will always be less than those of IB. A study of the relation of the two eyes and the two hemispheres under our conditions yields no conclusions which invalidate the comparison of IB with IIA.

*Extent of Central Fatigue.* We have shown that there is evidence for central 'fatigue' or inhibition however slight and unstable it may be.[79] We are now ready to consider the question of its extent. Our data give no unambiguous answer to this question. But let us see what may be found.

TABLE IX

| *O* | Stimulus Value | IIA | | | IIB | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2750 | 122 | 1.15 | .054 | 105 | .94 | .040 | .21 | .067 | 3.13 | .982 |
| H | 1500 | 25 | 1.04 | .032 | 63 | 1.02 | .024 | .02 | .040 | .50 | .632 |
| G | 1475 | 55 | 3.45 | .345 | 117 | 3.05 | .218 | .40 | .408 | .98 | .645 |
| Ha | 4900 | 131 | .80 | .021 | 139 | .51 | .008 | .29 | .022 | 13.19 | 1.000 |

The question whether the inhibitory effect extends throughout the visual center of the hemisphere involved may be tested by a comparison of the results from IIA and IIB. In both cases the same retina is involved, but in IIB two hemispheres. If the inhibitory effects spread in the same hemisphere, the invisibility periods in IIA should give an average greater than in the case of IIB. Likewise, the averages for IIIA should be greater than those for IIIB. A third test should be a comparison of $IIAC_1$ and $IIAC_2$. The former should give greater averages than the latter. Tables IX, X, and XI do not exhibit these necessary conditions. Yet the 4 *O*s agree that the averages for IIA will exceed those for IIB—2 of them with certainty and 2 with some doubt. In Table X one *O* is certain to give a positive difference while the other 3 give negative differences. Although the second test might be questioned, because it brings in the two different eyes, the third test is crucial and cannot be neglected.[80]

[79] We discovered during the course of the experiments an important verification of this fact in a qualitative study of a similar nature by K. Dunlap (Light spot adaptation, *Am. J. Physiol.*, 55, 1921, 201–211). F. Allen (Reflex sensations in vision, *J. Opt. Soc. of Amer.*, 7, 1923, 583 ff.), finds, however, just the opposite effect with intense stimuli. White light in one eye sensitizes—as Allen finds—the other, especially in the red, green, and violet. Although he has no evidence for a similar facilitory effect of weak light, he believes that "the reflex effects excited by them are proportionately large—even disproportionately large—with intense colors." (See also by the same author, On reflex sensations and color contrast, *ibid.*, 913 ff. Earlier studies which bear on the same fact are by E. B. Titchener (Über binoculäre Wirkungen monoculärer Reize, *Phil. Stud.*, 8, 1893, 231–309), and S. I. Franz (*Psychol. Monog.*, 12, 1899, 44 ff.).

[80] Professor Dunlap presents evidence to show that the inhibitory effect does spread to neighboring functional parts of the same hemisphere (*ibid.*, 210).

We may next ask whether the two hemispheres facilitate or inhibit each other. Again there is no unequivocal answer in our data. Tests of this question consist in comparisons of the data from IIA with IIB; of IIIA with IIIB. If there is facilitation the averages from IIA should be greater than those from IIB; those from IIIA should exceed those from IIIB. By facilitation we mean here that a removal of a stimulus affecting one hemisphere favors receptivity of a stimulus affecting the

TABLE X

| O | Stimulus Value | IIIA N | IIIA Av. | IIIA P.E.Av. | IIIB N | IIIB Av. | IIIB P.E.Av. | Diff. | P.E.D | D/P.E.D | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| D | 2750 | 148 | .90 | .007 | 239 | .98 | .026 | −.08 | .027 | 2.97 | .977 |
| H | 1500 | 70 | 1.02 | .010 | 181 | 1.57 | .069 | −.55 | .069 | 7.97 | 1.000 |
| G | 1475 | 121 | 1.98 | .084 | 172 | 3.76 | .243 | −1.78 | .257 | 6.93 | 1.000 |
| Ha | 4900 | 109 | .69 | .019 | 244 | .55 | .009 | .14 | .020 | 7.00 | 1.000 |

TABLE XI

| O | Stimulus Value | $IIAC_1$ N | $IIAC_1$ Av. | $IIAC_1$ P.E.Av. | $IIAC_2$ N | $IIAC_2$ Av. | $IIAC_2$ P.E.Av. | Diff. | P.E.D | D/P.E.D | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| D | 3250 | 23 | .72 | .029 | 22 | 2.04 | .360 | −1.32 | .361 | 3.66 | .994 |
| H | 1150 | 19 | .94 | .057 | 26 | 1.12 | .027 | −.18 | .063 | 2.86 | .973 |
| H | 1500 | 24 | .85 | .041 | 21 | .76 | .035 | .09 | .054 | 1.67 | .674 |
| G | 1750 | 77 | 1.82 | .109 | 39 | 2.73 | .261 | −.91 | .283 | 3.22 | .985 |
| Ha | 4900 | 41 | .51 | .013 | 89 | .75 | .019 | −.24 | .023 | 10.43 | 1.000 |

TABLE XII

| O | Stimulus Value | $IIIBC_1$ N | $IIIBC_1$ Av. | $IIIBC_1$ P.E.Av. | $IIIBC_2$ N | $IIIBC_2$ Av. | $IIIBC_2$ P.E.Av. | Diff. | P.E.D | D/P.E.D | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| H | 1500 | 15 | 1.17 | .207 | 42 | 1.05 | .052 | .12 | .213 | .56 | .697 |
| G | 1475 | 23 | 2.22 | .246 | 83 | 1.14 | .069 | 1.08 | .255 | 4.24 | .997 |

other. This would probably mean mutual inhibition of the two hemispheres for simultaneous stimulation. Further evidence would be forthcoming if the averages from $IIIBC_2$ were greater than those from $IIIBC_1$ (see Table XII). We have already found the first two tests to be conflicting in their tendencies (Tables IX and X) and the third test is negative in the case of the 2 *O*s for whom there are data. Indeed, if we were to base our conclusion upon the last two tests, we should have to say that the two hemispheres mutually inhibit each other for successive stimulations and facilitate each other for simultaneous stimulations. This conclusion would hardly be safe in view of the lack of data in the third test. The second test, the comparison of IIIA with IIIB, however, is so unanimous for 3 *O*s that it deserves special consideration. What other meaning it may have we are unable to say.

A final problem can be tested by our data, although it has little significance for fluctuations. It concerns the retinal con-

sequences of local stimulation with weak stimuli. Does stimulation of a retinal area facilitate or inhibit succeeding stimulation of other areas? We have Heyman's law to the effect that simultaneous stimuli inhibit one another. According to this law, we might expect facilitation from the removal of the first stimulus upon the second. If this were true, the averages in case of IIIA should be greater than those of IIA; those in IIIB than those in IIB; and those in $IIAC_2$ than those in $IIAC_1$. The data are favorable to this conclusion (see Tables XIII, XIV, and XI), except in the case of Table XIII. Here again the relatively low averages for IIIA upset the otherwise perfect correlation.

TABLE XIII

| O | Stimulus Value | IIA | | | IIIA | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2750 | 122 | 1.15 | .054 | 148 | .90 | .007 | .25 | .055 | 4.55 | .999 |
| H | 1500 | 25 | 1.04 | .032 | 70 | 1.02 | .010 | .02 | .033 | .61 | .659 |
| G | 1475 | 55 | 3.45 | .345 | 121 | 1.98 | .084 | 1.47 | .357 | 4.12 | .997 |
| Ha | 4900 | 131 | .80 | .021 | 109 | .69 | .019 | .11 | .028 | 3.93 | .995 |

TABLE XIV

| O | Stimulus Value | IIB | | | IIIB | | | Diff. | P.E.$_D$ | D/P.E.$_D$ | P.C. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | N | Av. | P.E.$_{Av.}$ | N | Av. | P.E.$_{Av.}$ | | | | |
| D | 2750 | 105 | .94 | .040 | 239 | .98 | .026 | .04 | .048 | .83 | .712 |
| H | 1500 | 63 | 1.02 | .024 | 181 | 1.57 | .069 | .37 | .073 | 5.07 | .999 |
| G | 1475 | 117 | 3.05 | .218 | 172 | 3.76 | .243 | .71 | .326 | 2.18 | .929 |
| Ha | 4900 | 139 | .51 | .008 | 244 | .55 | .007 | .04 | .011 | 3.64 | .992 |

If we make a general comparison of the averages from the 6 variations and then rank them for all 4 *O*s combined, we shall find them coming in the following order: IA, IB, IIA, IIIB, IIB, IIIA. If IIIB and IIIA were interchanged we could easily conclude that the nearer the second stimulation comes to the first, peripherally or centrally, the more is it inhibited by the first. This certainly holds so far as identical positions in the retina (IA) and in the projection center (IB) are concerned. IIA also shows a somewhat longer delay in reaction to the second stimulus as compared with the three remaining variations. Whether this continued inhibition from the first stimulation to the second neighboring stimulation exists and whether it is central or peripheral is a question which we have not answered. If such an inhibitory effect can be shown, as Dunlap thinks, it would be contrary to Heymans' law, unless this law applies only to the retina. Our other variations, IIB, IIIA and IIIB and controls, deny any uniformity of differences which can be interpreted as either central or peripheral inhibition for neighboring successive stimuli. We are for the present inclined to the conclusion that the central fatigue or inhibition is quite limited in its extent.

If one does not wish to base a conclusion of central fatigue upon averages from results which are so variable, there is still another method of comparison. We may ask ourselves in the case of IB what percent of the reactions to the second stimulus actually showed effects of central inhibition; *i.e.* what percent are certainly longer than the *normal* reaction-time to such stimuli. In order to find the normal reaction-times of our *O*s to the selected stimuli we may take their reactions to these stimuli under the conditions of IIA, IIB, and $IIAC_2$, disregarding any

TABLE XV
Percent of Delayed Returns

| *O* | IA | | IB | | IIIA | | IIIB | |
|---|---|---|---|---|---|---|---|---|
| | % | P.E. | % | P.E. | % | P.E. | % | P.E. |
| D | 82.2 | 2.3 | 16.4 | 1.5 | 0.0 | 0.0 | 5.6 | 1.0 |
| H | 75.7 | 1.5 | 36.4 | 1.3 | 1.4 | 1.0 | 10.6 | 1.7 |
| G | 30.5 | 2.6 | 15.5 | 2.1 | 0.8 | 0.6 | 5.3 | 1.1 |
| $H_2$ | 81.0 | 1.4 | 5.1 | 0.7 | 2.5 | 1.0 | 0.0 | 0.0 |

inhibiting or facilitating effects. Adding to every average reaction-time three times the standard deviation, we established the approximate upper limit of the normal reaction-time to every stimulus-value and used this as a criterion of delayed reactions. Any reactions in IB which are longer than this criterion are therefore certainly delayed.

Table XV shows the comparison of the various arrangements in the case of every *O*. The delayed reactions (percent) are lumped together for every *O* because every stimulus-value has its own criterion. IIA and IIB are omitted from the table since they were used in getting the criteria. Their percentage of delayed reactions should be approximately zero. The results demonstrate that from 5–35%, depending upon the *O*, of the reactions in IB are certainly delayed, and many more than this may be so delayed, owing to central inhibition.

Is there any evidence for a rhythm of receptivity such as one should expect along with the breathing and Traube-Hering waves? Yes, but only in the fact that the second stimulus is so unequally reacted to under the same kind of presentation. The light is not always equally perceptible even when an entirely new retino-cortical pattern is stimulated as in IIIB and $IIIBC_2$. To determine whether the physiological rhythms are responsible, one should have a record of the waves along with reaction-times to liminal stimuli, to see whether the short reaction-times coincide with certain phases of the rhythms.

But there are other possible explanations of the irregularity in reaction-time. One is the interference of 'retinal lights,' mists, and hazes which come into the field and influence receptivity to a marked degree. The *O*s' verbal reports are filled with such cases of interference. Another possible factor is eye-movement. Whereas hazes and mists may account

for delayed reappearances in IB, eye-movements may be responsible for immediate returns. Both factors, so far as can be determined, operate equally in nearly all the variations, however, so that our previous comparisons will still hold.

It was suspected, however, in the case of those *O*s for whom the stimulus nearly always returned immediately under the IB conditons, that eye-movements might be responsible. Although *O* could not possibly see the shutter move and it was practically noiseless, the change of stimulation might produce eye-movements. Attempts to photograph eye-movements during the IB series were successful in only one case. The difficulty was, of course, that with the blue photographic light in the *O*'s left eye there was a lowered sensitivity in that eye. The limens for the two eyes would be entirely different. With H we used the ultra-violet filter for the photographic light. This reduced the difference to a minimum, but still there was some difference, both in intensity and in quality. We equalised the white stimulus-light for the two eyes by placing wedge-shaped filters of gray and amber before the right eye. The amber was necessary because the stimulus-light appeared yellowish, by contrast, to the left eye. *O* reported that so far as he could detect the conditions were equivalent to the earlier ones in IB. The results show no unusual eye-movements which can be attributed to the action of the shutter.

With all the *O*s we were able to apply a more rigid test of fixation using procedure IIB. Here the stimulus is presented to the left eye alone and the light appears to jump from left to right, or right to left, when it reappears. Some records show steady fixation throughout this kind of series (see Plate II, $D_1$, $W_2$, and $W_3$) whereas most of them show unusual tremors at the moment of reappearance, that is, when the phenomenal movement occurs (see Plate II, $D_5$). Only in a few cases are there unusual tremors at the moment of disappearance which might be attributed to the movement of the shutter. These movements might mean an oculomotor flux which could have its effects on the visual cortex, removing fatigue effects and working for greater receptivity. Nowhere in the preceding section, however, have we found evidence that tremors do signify such conditions favorable for immediate receptivity.

Since the invisibility periods differ widely under the IB conditions, even when the field is free from mists and hazes, and since there may be immediate returns without eye-movement, we are led to the conclusion that this central inhibition is a very unstable, transitory sort of thing. This is just what one would expect if the all-or-none law of nervous conductivity holds for the simple cortical patterns involved here. Certain phenomenological descriptions, to be treated in the next section, also point in the same direction. We may therefore venture the following conclusions from this section. (1) In fluctuations the liminal stimulus produces a central fatigue or inhibition, a reduced readiness to respond to the same stimulation. (2) This fatigue is relatively local, probably limited to a small part of the optic sector. (3) This fatigue appears to be very unstable and of the all-or-none type.

## V. Does Attention Fluctuate?

What can we say regarding the general problem? Are these fluctuations with liminal visual stimuli to be regarded as fluctuations of attention as they have been called historically? If the question means what it has usually meant our answer must be a flat negative. According to this meaning of the question we must ask whether the fluctuations are manifestations of a fluctuating attention or a rhythm of attention, or are manifestations of extra-attentional factors. Our results, in agreement with some of the earlier studies, demonstrate an abundance of extra-attentional factors. Our demonstration that the fluctuations fit into the phi-gamma hypothesis suggests the operation of a number of factors rather than one. The demonstration of peripheral factors, such as local retinal adaptation, eye-movements and accommodation, is a good beginning toward an enumeration of these several factors. It will be recalled that those who hold the above meaning of the question accept the presence or absence of peripheral factors as a crucial test. It is also difficult to explain why the rhythm of attention should change with the change in intensity of the stimulus.

Suppose again that we take the two-level conception of attention. As we indicated in the very beginning, fluctuation of attention then may mean one of two things. It may mean a shifting of content in the two-level pattern, a redistribution of clearness in which one element, say our circular light patch, may periodically suffer to the extent of extinction, or it may mean a periodic loss of the degree of attention, of the difference between the upper and lower levels. If the latter type of change rests upon a general cortical receptivity, as indeed it must, as for example that condition supposed to be produced by a physiological rhythm such as breathing or the Traube-Hering waves, then again we cannot call the phenomenon a fluctuation of attention, because the same phenomenon has been shown to be occasioned by too many local factors. We cannot, of course, deny that there may be such a fluctuation of attention, that is, one dependent upon general physiological rhythms, but certainly the fluctuation of a minimal visual experience is not a simple manifestation of it.

If by fluctuation of attention is meant a shift in the pattern of clearness affecting changes in the relative clearness of mental contents (this change usually falls however, under the phrase 'shift' of attention or 'distraction' of attention), again the answer to the general question must be in the negative. It is even doubtful whether 'shifts' of attention have any more influence upon the fluctuation of minimal experiences than they have upon the average phenomenon which requires careful discrimi-

nation. That is the import of the few verbal reports in which the *O*s made any mention whatever of their own attention during fluctuations.

D reported as follows on a number of occasions: "I noticed during the first part of the experiment when the stimulus was present in the field, that I could direct my attention variously without in any way affecting the stimulus. I directed my attention to the biting-board, to the head-rest, to the light that seems to come in from the left side of the room, which I never noticed before. In spite of the fact that they seized my attention the stimulus persisted. . . . I have noticed this many times before but have neglected to report it." "I noticed no correlation whatsoever between the shifts of attention and appearance and disappearance of the stimulus. . . . After it has disappeared I can, on the contrary, attend away from all those things and direct my whole efforts to the expecting and waiting for the stimulus, but to no avail." Ha reported: "My attitude toward the stimulus is passive. I think the stimulus is at all times in the lower level of attention. My active attention is given to keeping my eyes on the fixation point." In the latter case a shift of clearness would probably *favor* the stimulus-light and hence would not cause a disappearance.

*Evidence from Phenomenology.* The question may be decided upon the basis of phenomenology. One may contend that the conditions which underlie the fluctuations are unimportant in answering the question; that the phenomenon can be called fluctuation of attention regardless of whether the physiological conditions are peripheral or central, general or local. But the more specific question which we now have to answer is whether the attributive changes which occur during fluctuations are at bottom a matter of clearness or attensity. While we are examining the descriptive reports we shall also notice what additional evidence they contain for the conclusions already reached and for some of the older theories, in particular the streaming phenomenon of Ferree.

The manner of disappearance of the faint patch of light is by no means identical in all cases; but the typical procedure includes qualitative, intensive and extensive changes. The qualitative changes consist in a change in the direction of the background, whether the latter is the more or less clear dark gray field or is a mass of formless haze or light gray mist which often occupies the center of the field. The spot and the background are often said to approach each other in quality and to fuse into a homogeneous ground. Intensive and extensive changes are most often characterised as a fading out, or a thinning out or loosening up in texture, together with a loss of contour and some spreading, or else a contracting of the spot into a harder, smaller, more flame-like mass which flickers out. The disappearance may be a slow gradual fading out, or a sudden flickering out, or both—that is, a gradual fading, cutting off abruptly at the end. The following reports testify to this generalized description.

Ho. "There is a course of disappearance; the very end comes rather abruptly. Sometimes before this there is a darkening, fuzzing over, a diffusing of gray with darkness. It becomes weak, diffuse, loose. Sometimes it goes out gradually, just becoming more and more diffuse, but often at the last it snaps out."

"It seemed to fade very gradually for a short period, then disappear all of a sudden as though some screen had intervened between my eyes and it, or as if it had been suddenly immersed in some opaque material. . . . At times there is the appearance of a flame in the stimulus. It seems to waver and shimmer and to disappear by way of a flicker."

Di. "There was a fluctuation of the more and the less intense; of the more and less definite boundary." "It expands, becomes loose, until it is no more than a very expanded luminous mist, then it is gone as the mist dissolves and disintegrates when the sun shines upon it."

D. "The spot changes considerably both in quality and in texture. The spot becomes more and more vapory until it cannot be distinguished from the retinal lights with which it fuses. This sort of disappearance I will designate as a fadeout; the other kind that I mentioned might be called a snapout." "The spot sank to the lower left, winking and blinking along its way until complete disappearance."

H. "The stimulus has two characteristic ways of coming in and disappearing. First it may come in or disappear suddenly, on the all-or-none principle. The other way is when it fades out gradually into the background."

One cannot be sure of the meaning of these two characteristic ways of disappearing, but they must mean the operation of different factors. If one wished to theorize on the basis of what has gone before, one might well attribute the gradual fading to retinal adaptation and the flickering and sudden disappearance to neural inhibitions of an all-or-none type. It is significant from the phenomenonological point of view that not a single direct report of attensive changes can be found. This may well be due to the fact that the *O*s are rarely called upon to make such reports and had no specific instructions to do so here. On the other hand no specific instructions were given for any attributive description and the *O*s knew that the problem lay in the field of attention. The instructions given were merely to "describe the temporal course of your experience."

The reappearance is more often sudden and since it has a short duration descriptions are less complete. Attributive changes are approximately the same as for disappearance, except that they are reversed in time. Sometimes there is a kind of hint of light by way of warning and then a rapid appearance. At other times there is an unexpected return to full intensity. The latter case, as we have mentioned before, frequently follows eye-movements. The following descriptions are typical.

Ho. "When first I get it after disappearance, the reappearance is of very short duration; a few rapid fluctuations in and out. In nearly every case it comes in conversely to the way in which it went out." "The return of the light gray is not uniform. It is usually one of two kinds. Either at first I get a very, very faint hint of light gray quality out there, sort of a

halo effect, and then with a little more search around, the light gray comes. The other is that the light gray pops in suddenly with full extent and full intensity."

H. "Again it seemed as if the coming in of the stimulus was presaged by or signaled by a general lightening up of the haze about the fixation point. As it came in the field got darker and it stood out in bold relief."

There were a few reports of the objective and subjective fluctuations first pointed out by Eckener. They are best described by H.

"Sometimes the stimulus seems to disappear in a wholly objective way, that is, the stimulus itself goes out; something out there goes out. At other times it seems as if the stimulus were still there but something has happened to me so that I can't see it. In the first case the stimulus goes out in a clear, definite, objective fashion; I notice no change within myself; I am seeing something and then seeing something else. In the other case it seems as if something happens to me to prevent my seeing the stimulus. In this case it goes out more gradually."

It is not easy to say what the meaning of this report is. That the two types of fluctuation should carry the two meanings of objectivity and subjectivity must be a sign of some fundamental difference. What the difference is remains a problem.

*The Streaming Phenomenon.* Although we made no specific studies upon Ferree's streaming phenomenon,[81] which formed a part of his theory of fluctuations, numerous reports indicate the influence of various entoptic processes upon the weak stimulus-light. The field is occasionally full of various phenomena which sometimes cut across and obliterate the stimulus-light. The following reports are typical.

Wa. "I did not get the effect of the white space moving to the side, but of a screen; a portion of the field came in between and blotted it out." "As a rule its obliteration took the form of innumerable entoptic patterns. At times they were almost as bright as the stimulus itself." "The moving film appeared as a series of waves of darker portion and lighter portion bebetween moving downward and to the right." "The fluctuation was preceded on each occasion by a very definite concentric closure of a black screen, much like the shutter of a camera. It did not appear to be on a plane surface but on a spherical or concave surface. The blackness of the screen was very intense."

Ho. "The disappearance almost always seemed to be a blotting out by way of other visual qualities which covered up the light gray quality."

G. "Through most of the period there were vertical bands or horizontal bands cutting across, sometimes moving and sometimes not, sometimes obscuring the exposure-light."

D. "It disappears in two ways; sometimes suddenly, at other times more slowly, as if washed over by retinal lights."

H. "It seemed as if something moved across the stimulus many times when it went out."

These streaming movements are also confirmed indirectly by the movement of the stimulus-light itself. This is known as Charpentier's illusion or as the autokinetic sensation of Au-

[81] Ferree, this JOURNAL, 19, 1908, 129 ff.

bert. All *O*s reported this phenomenon on numerous occasions. The movement usually takes a direction intermediate between the horizontal and vertical axes of the field.

It was suggested by one of the *O*s that the movement of the stimulus-object is apparent because the greater part of the visual field is streaming in the opposite direction.[82] The *O* refers the movement to the smaller object, just as when the moon appears to move behind floating clouds. This state of affairs is implicitly described in the report of W when he says, "The stimulus seemed to be moving in a downward right direction, behind a veil of some opaque material. It almost covered up the stimulus, yet I had the impression that the stimulus was moving." It is significant, too, that the direction of the movement is identical with the most usual directions of the streaming movements found by Ferree.

There is nothing in any of the reports, however, which indicates a connection between eye-movements and the streaming processes; but the *O*s had no instructions to report upon this matter. All reports indicate a connection between the streaming phenomena and disappearance, but we have seen (p. 560) that the periods just before disappearance are peculiarly free from eye-movements. There is a lack of verbal statement indicating a connection between the streaming phenomena and reappearance. This does not, of course, disprove a connection, and on the positive side we have found that eye-movements are most numerous just preceding reappearances. If Ferree's streaming phenomena are manifested by the mists and hazes which infest the field and obliterate the stimulus, there is some evidence for the connection, because these hazes are most numerous when the *O* reports extremely good fixation. Good fixation, then, favors the formation of hazes, and eye-movements may have the effect of clearing the field by producing an acceleration of the streams upon the retina. In this sense Ferree's theory seems to be essentially correct. But this is not the only connection between eye-movements and reappearance, nor is local retinal adaptation *plus* streaming movements adequate to a complete explanation of fluctuations, as we hope the earlier sections have demonstrated.

## VI. Summary and Conclusions

In this paper we have attempted first of all to set the problem of fluctuations with liminal visual stimuli in its proper historical light; to show that this phenomenon is nothing more than a matter of the limen; to discover the ways in which the phenomenon is dependent upon the intensity of the stimulus; to point out the operation of certain peripheral and central physiological factors; and lastly, to decide the general question whether the phenomenon is to be regarded as a fluctuation of attention. We have been led to the following conclusions.

[82] We are indebted to Dr. Dallenbach for this suggestion. Further studies in preparation will test this observation.

(1) Fluctuation with liminal visual stimuli is a phenomenon which is connected with all liminal stimuli, as attested by (a) the fact that it agrees with the phi-gamma hypothesis, and (b) the fact that the stimulus value which gives 50% visibility during fluctuations is near the liminal value which is found in the usual manner if we make allowances for local adaptation.

(2) The length of the periods of visibility and invisibility and the total period, are functions of the intensity of the stimulus.

(3) There are a number of physiological conditions, both peripheral and central, involved. Among them are (a) local retinal adaptation, (b) eye-movements, and (c) local central fatigue or inhibition.

(4) Neither from psychophysical evidence nor on the basis of phenomenology can this phenomenon be called a fluctuation of attention.

# COLOR SEQUENCES IN THE AFTER-IMAGE OF WHITE LIGHT

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## I. INTRODUCTION

The procession of colors which occurs in the after-image of a strong white light has been a source of interest to many observers. The phenomena were described by Helmholtz under the title *Farbiges Abklingen der Nachbilder* and are referred to by English writers as "the flight of colors." An extended resumé of the literature was given by the present writer in the *Psychological Bulletin*,[1] so a review is not given here. It was shown there that descriptions of the phenomena which have been given by various writers have differed considerably. Recent contributions to our knowledge of the subject have been made by Miss Shuey.[2]

The observations reported in this paper were made in an attempt to discover whether there is a relative constancy in the character of the color sequence. The problem grew out of the consideration of the fact that the conditions of observation in previous work had not always been even approximately constant and comparable. In this experiment the attempt was made to control as much as possible the conditions of observations for every *S* in the group.[3]

## II. EXPERIMENTAL PROCEDURE

The apparatus used was set up in a room from which all light from external sources was excluded. The essential feature of the apparatus was a light-tight box, suitably wired, having a circular aperture in the front wall. The dimensions of the box were 3 ft. in length, 11 in. in height, 10 in. in width. The whole arrangement was elevated so as to bring the center of the circular aperture on a level with the *S*'s eyes. The aperture had a diameter of 5 cm. The source of light was a 100 watt tungsten lamp placed at a distance of 30 cm. behind the aperture. A plate of milk glass was inserted between the lamp and the plate in order to diffuse the light. With the size of aperture used in this series of observations there was a light surface which was regarded by all the *S*s taking part as homogeneously bright. A curtain was mounted on the top of the box and slightly forward of its front wall. This curtain was released simultaneously with the extinction of the light, and in falling effectually prevented any prolongation of the stimulus during the cooling of the filament. At the rear of the box was a

[1] W. Berry, The flight of colors in the after-image of a bright light, *Psychol. Bull.*, 19, 1922, 307-337.

[2] A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 557-582; The effect of varying periods of adaptation on the flight of colors, *ibid.*, 37, 1926, 528-537.

[3] The observations here reported were completed in the Psychological Laboratory of the University of Chicago, under the direction of Professor Harvey Carr, in 1922. An abstract of the results appeared in the University of Chicago Abstracts of Theses, Humanistic Series, I, 1924. The writer takes this opportunity of thanking Professor Carr for his interest in the work and for his stimulating influence throughout its course. Thanks are also given to the persons whose patience and good will in coöperating with the writer made these observations possible.

small chamber in which the *E* made a record of the reports of the changes in the image as they were announced by *S*. This was lighted by a small dry-cell lamp, the whole being heavily curtained to prevent any leakage of light.

The method of procedure in every case was the same. *S* sat in a chair directly facing the light. The distance from the light surface to the eyes was 150 cm. A dark adaptation period of 10 min. was allowed. Shortly before the expiration of the period, and before the light was exposed, a metronome was started, beating 60 to the minute. Every fourth beat a bell was rung. The light was exposed for 60 sec., during which period *S* steadily fixated the center of the light surface. (A constant intensity of stimulus and a constant size of stimulus were used in the observations reported in this paper.) The light was then extinguished and the fall curtain released. This coincided with one of the periodical strokes on the bell and served to mark the beginning of the secondary period, the period of the after-image. *S* then began to report the changes in the after-image, using a system of description previously agreed upon and described in a subsequent paragraph. *E* immediately indicated on record cards the reports made by *S* during each period of 4 sec. This provided a means of measuring the duration of the flight and a record of the qualitative changes which were observed. Only one observation per day was made by any *S*, the observation being made, whenever possible, at the same time each day. All the *S*s were graduate students in the Department of Psychology.

The question of terminology was rather troublesome. All the *S*s were well acquainted with the difficulties inherent in an experiment involving the use of color terms. From the outset the convention of using 'simple' color terms was adopted. These were defined as the usual names given to the well-known divisions of the spectrum, together with such descriptive terms as seemed necessary to complete the report. These were 'dark,' 'bright,' 'saturated,' etc. Compounds such as 'yellow-green,' 'blue-green,' etc., were included. All the *S*s had a practice series of observations, in order to accustom them to the rather fugitive character of the phenomena, and to enable them to acquire some proficiency in reporting the progress of the changes. Twelve *S*s took part, including the writer.

On account of the fugitive character of the image, and the fact that changes took place in two, three, or even four areas of the total image at the same time, the *S*s were instructed to attend strictly to only one of these areas, and to report the changes there. The material presented in this paper is on the flight of colors in the center of the image. This area corresponds to the one described by Homuth[4] as *Bildkern*, only in this instance the form of the colored mass was circular, corresponding to the circular light surface. Fechner,[5] Séguin,[6] and Homuth have called attention to the advisability of this method of procedure in the interest of a clearer and more accurate description of the phenomena.

## III. Results

(*1*) *The General Character of the Color Phenomena in the Image.* It is, of course, impossible to present all the data gathered on the characteristics and content of the color sequences for every *S* through a series of observations. Realizing this, I have attempted to represent graphically several typical records of the observations made in this investigation. The data obtained were transferred to a scale and the behavior of the images charted in the following manner. The vertical line represents the order of colors

[4] P. Homuth, Beiträge zur Kenntnis der Nachbilderscheinungen, *Arch. f. d. ges. Psychol.*, 26, 1913, 181-268.

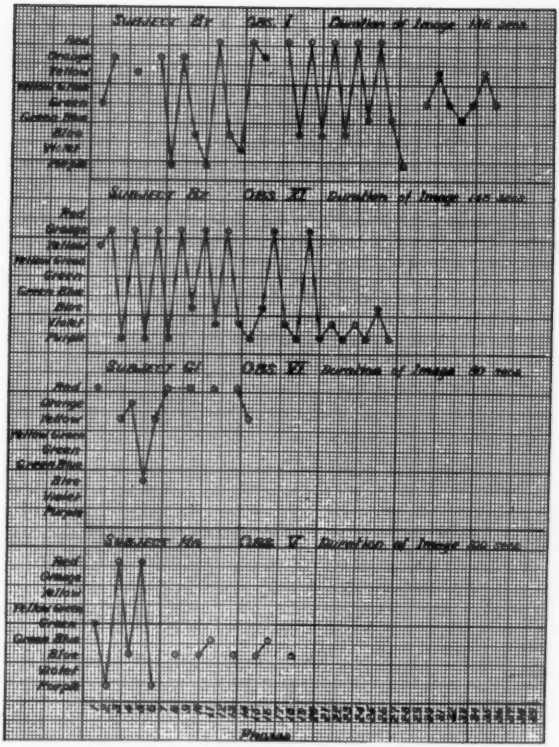
[5] G. Th. Fechner, Über die subjektiven Nachbilder und Nebenbilder, *Pogg. Ann.*, 50, 1840, 445-470.

[6] J. M. Séguin, *Ann. de Chimie et de Physique*, Ser. 3, 41, 1854, 413-431.

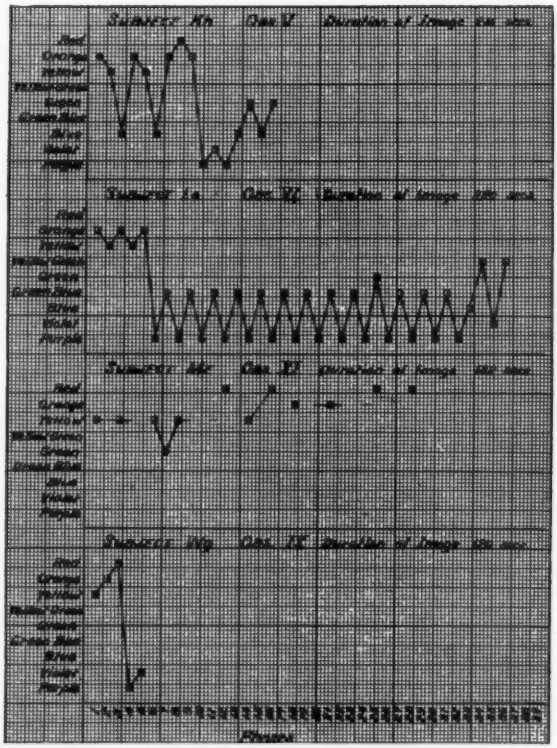
from red at the top to purple at the foot of the line. The horizontal line represents the order of phases in the flight of colors. A 'phase' is defined as either the appearance of another color or a gray (achroma) in the area of the image being observed, or the disappearance of the image altogether. A point on the chart represents the color which was reported in a given phase. Where there was continuity in the color sequence of the image, the points are joined by a straight line. A break in the line represents a phase in which the image was reported "gone." The appearance of a gray (achroma) phase is indicated by the dotted lines joining two points. Several examples of these charts are given on pp.587-8. The first chart, *e.g.* may be read as follows: the first phase reported was green, followed by orange. Here there was a break in the continuity, the image being reported as "gone." This was followed by yellow, another break appearing at that point. The sequence of colors is then given, orange being followed by purple, and so on through the entire flight. It must be clearly understood that this method of representing the course of the image is quite arbitrary. The charts are intended to show only the sequences from phase to phase in the images. There is nothing quantitative in the charts, the lengths of the lines joining the points depending entirely on the arbitrary pattern of the scale adopted. It is believed by the writer that this method has certain advantages over other methods of showing the course of the color changes. The charts, not all of which are reproduced here, show some striking features which will be discussed in the succeeding paragraphs.

Taking the results as a whole, all the spectral colors were present in the images, together with purple and gray. The colors which appeared most frequently, irrespective of the places they occupied in the flights, were purple, blue, and red, the colors yellow, orange, and green following in this order, with slight differences in their relative frequencies of appearance. The colors which appeared least frequently were yellow-green and violet. Of the 12 *Ss* in this group, two reported all the spectral colors in their observations. This statement does not mean that all the colors were present in any one of the flights. Two *Ss* reported only 5 colors in the entire series. Two *Ss* reported red as the most frequently seen color in the series, two reported yellow, one reported green, three gave blue, one gave violet, and three gave purple. All the *Ss* reported the appearance of yellow, blue, and purple, although with different frequencies. Kr failed to give a single report of red or green. Gl did not report green at all. Mr and Wg gave it once only. Very noticeable individual differences in the frequency of the appearance of gray and the disappearance of the image during the flights were noted. One *S*, Mr, regularly reported the appearance of 'black' in alternation with some colored phase. This was the outstanding case in the entire series of the instability of the image and the regular appearance of non-colored phases. The latter amounted to almost 33% of the total phases reported, while the disappearances of the images amounted to 27%. On the other hand, there were two *Ss* whose records show that the images were remarkably stable and free from achromatic phases. These were Le and Wg, who did not give a single report of 'gray,' or 'gone.' These two *Ss* showed differences in the number of phases per flight; the former consistently had a relatively large number, the latter a relatively small number of phases. Kr and Kh showed relatively great stability in the images. Gl and Hn had images which were relatively unstable.

Further analysis of the records revealed the fact that the content of the flight varied with the different stages of the image. In all cases the images showed several phases, the number, however, varying with the individual. In order to show how the different stages of the images differed, every record of every observation was divided into 3 parts, irrespective of the number of phases in the flights. The total number of phases in each flight



CHARTS SHOWING THE COURSE OF THE COLOR CHANGES IN THE AFTER-IMAGE OF A BRIGHT LIGHT CETNRAL AREA



CHARTS SHOWING THE COURSE OF THE COLOR CHANGES IN THE AFTER-IMAGE OF A BRIGHT LIGHT CENTRAL AREA

TABLE I

Total Content of the Flights in the After-Images of a Bright Light. Figures Show the Number of Times a Given Color Appeared in the First, Second, and Third Divisions of the Course of the Flights (12 *Ss*, 12 Observations Each)

| *S* | Division | Red | Or. | Y. | Y-G. | Gr. | G-B. | Bl. | Vio. | Pur. | Gray | Gone |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| By | First | 23 | 21 | 2 | | 17 | | 12 | 10 | 8 | 9 | 2 |
| | Second | 29 | 2 | 8 | | 8 | 11 | 19 | 13 | 6 | 3 | 6 |
| | Third | | | 10 | | 49 | 16 | 3 | 1 | 15 | 7 | 24 |
| Bn | First | 7 | 6 | 3 | 2 | 7 | 4 | 1 | 9 | 9 | | 1 |
| | Second | 3 | 2 | 2 | 1 | 6 | 6 | 10 | 11 | 2 | | 7 |
| | Third | | 1 | 5 | 7 | 9 | 8 | 3 | 5 | | 1 | 13 |
| Bz | First | 18 | 27 | 6 | | 4 | 1 | 5 | 3 | 22 | | |
| | Second | 9 | 22 | | | 5 | 1 | 14 | 14 | 21 | | |
| | Third | 1 | 5 | | | 2 | 2 | 25 | 8 | 37 | 2 | 2 |
| Gl | First | 9 | 9 | 16 | | | | 6 | | 2 | 5 | 11 |
| | Second | 18 | 2 | 11 | | | | 3 | | 1 | 15 | 8 |
| | Third | 26 | 3 | 5 | | | | 1 | | | 19 | 7 |
| Hn | First | 18 | 1 | 2 | | 9 | | 4 | | 13 | 2 | 5 |
| | Second | 3 | | 1 | | 1 | 6 | 17 | 1 | 9 | 2 | 14 |
| | Third | | 1 | 1 | | | 7 | 20 | | 1 | 4 | 20 |
| Kh | First | | 17 | 10 | | 1 | | 6 | 7 | 7 | 1 | |
| | Second | 1 | 3 | | | 6 | | 15 | 6 | 18 | | |
| | Third | | 2 | | | 23 | | 15 | 3 | 6 | | |
| Kr | First | | 2 | 12 | | | | 1 | | 1 | 2 | |
| | Second | | 2 | 3 | | | 1 | 5 | | 1 | 6 | |
| | Third | | | | | | | 3 | | | 6 | |
| Le | First | 13 | 24 | 22 | 4 | 8 | 30 | 7 | 1 | 39 | | |
| | Second | | | | | 2 | 48 | 23 | | 75 | | |
| | Third | | | 2 | 19 | 1 | 32 | 39 | | 55 | | |
| Mr | First | | | 64 | | 1 | | 10 | | 2 | 44 | 28 |
| | Second | 6 | 12 | 28 | | | | 1 | | 1 | 56 | 45 |
| | Third | 18 | 35 | 2 | | | | | | | 50 | 49 |
| Ml | First | 12 | | 10 | 6 | 12 | 6 | 4 | 8 | 11 | 2 | |
| | Second | 5 | | 2 | 2 | 16 | 12 | 2 | 4 | 17 | 10 | 4 |
| | Third | 6 | | 2 | 1 | 11 | 6 | 2 | 1 | 13 | 14 | 8 |
| Rl | First | 5 | 12 | 3 | 3 | 9 | 1 | | | | | |
| | Second | 14 | 5 | | | 3 | 3 | 4 | | 6 | 1 | 2 |
| | Third | 4 | 1 | 1 | 1 | 7 | 10 | 10 | | 4 | | |
| Wg | First | 1 | 5 | 12 | | | 1 | 7 | | | | |
| | Second | 11 | 4 | 1 | | | | 2 | | 3 | | |
| | Third | | | | | 1 | | 7 | 1 | 10 | | |

of colors was ascertained, and the number divided by 3. This method gave a first, second, and third division of the flights, which were examined to determine the content in each division. Table I shows the result of this analysis. The numbers in the table indicate the frequency with which a certain color appeared in each of the three divisions respectively for various individuals. The same data for the group of observers as a whole are given in Table II.

TABLE II

Summary of Table I, Showing the Frequency of the Appearance of Each Color in Each Division of the Course of the After-Images for the Entire Group of *Ss*

| Division | Red | Or. | Y. | Y-Gr. | Gr. | G-Bl. | Blue | Vio. | Pur. | Gray | Gone |
|---|---|---|---|---|---|---|---|---|---|---|---|
| First | 106 | 124 | 162 | 15 | 68 | 43 | 63 | 38 | 114 | 65 | 47 |
| Second | 99 | 54 | 56 | 3 | 47 | 88 | 115 | 49 | 160 | 93 | 86 |
| Third | 55 | 48 | 28 | 28 | 94 | 81 | 128 | 19 | 141 | 103 | 123 |

From Table I, it is evident that there were some individual differences among the members of the group in respect to the content of the flight in each of the 3 parts of the course of the image. Every *S* seemed to have his or her own individual pattern in the behavior of the colored images, which persisted, moreover, through the entire series. For example, Mr showed a preponderance of the red and orange colors in the final part of the images, and there was a marked predominance of yellow in the first part. On the other hand, the red and orange colors dropped out completely in the second and third parts in the case of Le, and in these divisions the green-blue, blue, and purple colors were the predominant colors present. Another *S*, Bz, showed a marked grouping of the red and orange colors, on the one hand, and the blue and violet colors, on the other, in the first and final divisions respectively.

Table II seems to offer some slight evidence of a group tendency with respect to the behavior of the colors in the different divisions. Taking the combined results, it will be seen that there was a progressive diminution of the frequency of the red, orange, and yellow colors, and an increase in the frequencies of the colors green to purple, excluding violet. In addition, there was also a progressive increase in the frequency of the appearance of 'gray,' usually a very dark gray or black, and evidence of greater instability of the image in the later stages of its course.

TABLE III

Number of Times a Given Color Appeared in the Initial Phase of the After-Images (12 *Ss*, 12 Observations Each)

| *S* | Red | Or. | Y. | Y-Gr. | Gr. | Gr.-Bl. | Bl. | Vio. | Pur. |
|---|---|---|---|---|---|---|---|---|---|
| By | | 2 | | | 6 | | | | 1 |
| Bn | | 2 | 1 | 2 | 2 | 1 | | 2 | 1 |
| Bz | 3 | 3 | 5 | | 1 | | | | |
| Gl | 4 | 2 | 3 | | | | 1 | | 1 |
| Hn | | 1 | 1 | | 8 | | 1 | | 1 |
| Kh | | 4 | 4 | | 1 | | 2 | | |
| Kr | | | 12 | | | | | | |
| Le | | 5 | 3 | | 4 | | | | |
| Mr | | | 7 | | | | 3 | | 1 |
| Ml | | | 8 | 4 | | | | | |
| Rl | | 2 | | | 9 | 1 | | | |
| Wg | | | 4 | | | 1 | 7 | | |
| Total | 7 | 21 | 48 | 6 | 31 | 3 | 14 | 2 | 5 |

(2) *Initial and Terminal Colors.* In most cases the colors of the after-image were seen immediately after the light was extinguished. There were 7 cases in the series where brightness of a very high order was reported in the first phase, this being followed by some color. Table III shows the character of the initial phase in the flight for every individual. The figures indicate the number of times in 12 observations a given color was the first one seen.

It will be seen that only one *S* commenced the flight with the same color through the entire series of observations. The other *S*s showed more or less tendency to vary somewhat the initial color. Bn gives an especially good example of this tendency. Yellow was the most frequently seen first color, with green, orange, blue, red, yellow-green, green-blue, and violet following in that order. In his observations on the after-image, Franz wrote, "From the after-image experiments in the series of anthropometric tests . . ., it was found that the after-image when first seen was sometimes positive and sometimes negative; and the colors varied greatly, being distributed in the first place as follows: negative or dark, 33%; light or white, 29.4%; blue, 13.7%; purple, 9.8%; green, 5.9%; yellow, 3.9%; red, 2.0%; miscellaneous, 2.0%."[7] The individual variability in the flight of colors is brought out rather clearly by both groups of figures. Miss Shuey[8] has recently published some observations on the color phenomena of the after-image. She used 4 different intensities of stimuli, with an exposure of 40 sec. Her results, with the same intensity of stimulus as was used in this experiment, are as follows: "20 of the 55 images began with green, 8 with yellow, 6 with an 'unnamed' light, 6 with yellow-green, 6 with purple, 2 with blue, and 1 with red." Marked variability and individual differences seem to be a characteristic of the first stages of the color sequences of the after-image.

By 'terminal color' of the flight is meant the last color reported in an observation of the center disk or area of the image. Usually this part diminished in size to a very small circle of color seen against a dark, or unsaturated, colored ground. In some cases the final phases of the flight were made up of alternate disappearance and re-appearance of the colored center. Consequently it was decided to define the terminal color as that last seen, even though the *S* was able to report the presence of the image in other terms. One *S* always reported green as the terminal color; another showed a constant tendency to see yellow or yellowish-green last. Two *S*s saw the red and orange colors, or the red, orange, and yellow colors respectively, as terminal colors, and two always reported the blue, violet, and purple colors last.

(3) *Cycles of Color.* One of the primary purposes of this investigation was to obtain data on the so-called cycles of colors in the after-image. McDougall has called attention to and has described these features of the flight of colors in some detail. He says, "The two observations last described illustrate a very constant feature of such after-images, namely, the tendency of the three primary colours to follow one another in a recurring cycle of the order G, R, B, G, R, B."[9] He adds, "In the one case the cycle passes from G to G, in the other from B through G, R, B to G, again, thus almost completing the double cycle." The present writer has elsewhere pointed out that, "it is by no means easy to disentangle the facts from the account given by McDougall. Apparently he used different durations, as well as different intensities, but they seem to have been used in an irregular order."[10]

The data gathered in this investigation were carefully scrutinized for the evidences of recurrent cycles of colors in the images. In this report the

[7] S. I. Franz, On after-images, *Psychol. Monog.*, 3, 1899, no. 12, 39.

[8] A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 569.

[9] W. McDougall, *Mind*, N. S., 10, 1901, 240.

[10] Berry, *op. cit.*, 323.

term 'cycle' means the appearance of two or more colors in a sequence, which sequence is repeated one or more times. A cycle is called a '3-component recurrent cycle' when 3 colors in sequence are repeated one or more times in the same order. In like manner, a '2-component recurrent cycle' has 2 colors in a rhythm, repeated one or more times.

(*a*) *Three-component recurrent cycles.* Only a few cases of 3-component color cycles were found. Not one example of the recurrent sequence of red, green, blue was reported. There were 2 sequences of purple, red, blue. There was one example each of the following sequences respectively: orange, purple, blue; orange, yellow, blue; and purple, violet, blue. One example was orange, violet, purple, followed by red, blue, purple. These sequences were reported by but 4 *Ss* of the 12, 2 of the *Ss* giving 2 examples each.

If the total number of the reported phases in the entire series be taken into consideration, it will be seen that there is some justification for saying that these 3-component color cycles are evidently relatively rare. The total number of phases reported was 2543. Of these, 2026 were described by the use of one or other of the color terms. This number would provide at the maximum for 675.3 possible sequences of 3 colors each.

(*b*) *Two-component recurrent cycles.* This type of color cycle is made up of 2 colors in a sequence, which is repeated one or more times during the course of the flight. They will be referred to as the oscillation cycles. Eight of the 12 *Ss* reported this type. Two *Ss* reported them in each observation throughout the series, one *S* saw them in each observation but one, two *Ss* saw them in 6 observations, one *S* saw them in 5, another in 4, and another in 3 observations. Four *Ss* did not give definite evidence of the oscillation cycle of colors. Of these 4 *Ss*, one showed a definite tendency to see an oscillation between a colored phase and an uncolored phase, or a disappearance of the image. Another *S* reported a large number of 'partial' oscillation cycles, in which the colors were not exactly repeated. One example of this is the sequence red and violet, followed by orange and purple. There were 2 *Ss* who reported images in which there was no evidence of a cycle at all. There was a gradual transition from one color to another without repetition of any of the components in the sequence. Kr's records showed the following general changes in the course of the flight: yellow, orange, blue; the flight generally terminating in a dark gray or black. Wg's records showed the following general changes: blue which becomes lighter, yellow, orange which passes into reddish orange, red, reddish purple which becomes darker, passing finally to a dark blue. These two types of flights resemble strongly some of the flights reported in the literature. With these two exceptions, it may be said that the oscillation type of color change in the flight of colors was manifested, either completely or partially, throughout the entire series. There were, however, very marked individual differences in the 'pattern' of the cycles. Homuth has described a type of oscillation in the after-image which resembles somewhat the type found in this investigation. He distinguished two kinds of oscillation: (1) a regular or irregular disappearance of the after-image; (2) a rivalry between colors of the after-image extending over a smaller or larger part of the surface of the image. He says further, "Ich glaube übrigens, dass die erste Form nur ein Sonderfall der Zweiten ist."[11] As has been shown above, there were 3 *Ss* in this investigation who reported examples of what might be called the 'disappearing' oscillation cycle.

Some of the marked individual differences in the pattern of the cycle may be summarized as follows:

(1) There were differences in the relative number of the cycles. In some cases the sequence of 2 colors was repeated only once, making one

[11]Homuth, *op. cit.*, 222.

cycle; whereas in some other cases the sequence was repeated several times, thus completing several cycles. Table IV summarizes the data obtained. The number of cycles, the number of phases in the cycles, the total number of phases in the entire series, and the percent the number of phases in the cycles forms of the total number of phases, are indicated for every *S* who reported this type of color change, through 12 observations.

TABLE IV

| *S* | No. of Cycles of Colors in After-Images | No. of Phases in the Cycles | Total Number of phases in the Images | Percent |
|---|---|---|---|---|
| By | 27.5 | 110 | 325 | 33.8 |
| Bz | 27.5 | 110 | 256 | 43.0 |
| Gl | 3.0 | 12 | 177 | 6.8 |
| Hn | 4.0 | 16 | 162 | 9.9 |
| Kh | 14.0 | 56 | 147 | 38.1 |
| Le | 92.0 | 368 | 444 | 83.0 |
| Ml | 7.5 | 30 | 209 | 14.3 |
| Rl | 7.5 | 30 | 109 | 27.5 |
| Total | 183.0 | 732 | 1829 | |

TABLE V

| *S* | Colors | No. of Cycles | *S* | Colors | No. of Cycles |
|---|---|---|---|---|---|
| By | Red, blue | 11.5 | Bz | Purple, orange | 9.0 |
| | Red, violet | 7.0 | | Blue, purple | 9.0 |
| | Green, purple | 3.0 | | Purple, red | 4.0 |
| | Yellow, green | 2.0 | | Orange, violet | 3.5 |
| | Green, orange | 2.0 | | Green, purple | 1.0 |
| | Red, gr-blue | 1.0 | | Purple, violet | 1.0 |
| | Yellow, gr-blue | 1.0 | | | |
| Gl | Red, yellow | 2.0 | Hn | Purple, red | 2.0 |
| | Blue, yellow | 1.0 | | Green, red | 1.0 |
| | | | | Blue, purple | 1.0 |
| Kh | Green, blue | 5.0 | Le | Gr-blue, purple | 54.5 |
| | Purple, blue | 4.5 | | Purple, blue | 24.0 |
| | Green, purple | 1.5 | | Blue, y-green | 6.0 |
| | Orange, violet | 1.0 | | Orange, yellow | 4.5 |
| | Violet, green | 1.0 | | Orange, green | 1.0 |
| | Violet, purple | 1.0 | | Gr-blue, y-green | 1.0 |
| | | | | Purple, green | 1.0 |
| Ml | Purple, green | 4.0 | Rl | Red, gr-blue | 3.0 |
| | Purple, gr-blue | 2.5 | | Orange, red | 2.5 |
| | Yellow, red | 1.0 | | Red, purple | 1.0 |
| | | | | Orange, yellow | 1.0 |

(2) There were also differences in the character of the components of the cycles as well as in the frequency of their appearance. The distribution of the 183 cycles of the entire series according to the individual reports is indicated in Table V.

There were 22 combinations of colors, and the frequencies ranged from 1 to 54.5 cycles. The most frequent cycle was green-blue and purple,

but this, it will be noted, was found principally in the records of one *S*. The pattern of the flights for this *S* was that most nearly constant throughout the entire series. A very clear idea of it may be gained by glancing at the examples of typical flights represented in the chart on page 588. Excluding the records of this *S*, this combination of colors appeared in 2.5 cycles. Similarly, the pair of colors red and blue, appeared in 11.5 cycles, all of them in the records of one *S*. The pairs green and red, and blue and yellow, appeared respectively in only one cycle each.

(3) Although there were some exceptions, there is evidence to show that the oscillations were more numerous during the middle of the flights than at other periods.

(4) *The Duration of the Flight.* The attempt was made to determine the duration of the flight of colors. Although there are always certain difficulties in the measurement of such phenomena as the after-image, it was believed that some useful data could be obtained by the use of a simple method of measurement. This has already been described. To recapitulate, a metronome was started just before the light was exposed. This metronome rang a bell every fourth second, and the extinction of the light coincided with one of these strokes on the bell. The secondary period was measured by the number of periods which elapsed until the image disappeared. It was decided to define the end of the flight as coinciding with the end of the period in which the color was last reported in the observation. The figures are set forth in Table VI with the full knowledge that their value is approximate.

TABLE VI

| *S* | Median | Av. Dev. | Range | |
|---|---|---|---|---|
| | | | Upper | Lower |
| By | 126 | 14.3 | 156 | 112 |
| Bn | 118 | 29.6 | 200 | 58 |
| Bz | 146 | 10.0 | 172 | 124 |
| Gl | 86 | 20.3 | 144 | 60 |
| Hn | 96 | 13.3 | 140 | 60 |
| Kh | 134 | 31.1 | 212 | 104 |
| Kr | 94 | 20.3 | 140 | 52 |
| Le | 198 | 14.3 | 220 | 148 |
| Mr | 156 | 27.3 | 200 | 120 |
| Ml | 112 | 20.3 | 184 | 72 |
| Rl | 134 | 19.3 | 180 | 80 |
| Wg | 116 | 28.0 | 204 | 80 |

In several cases the after-image persisted as a colorless disk for some time after the colors had disappeared. Taking the figures as they stand, it will be seen that there is considerable variability. Sometimes an *S* would report a flight of long duration, which would be followed by one of short duration, which reverted to one of longer duration. Miles found 3 types of *S*s in his experiments: one in which there was very little variation from test to test, another in which the *S*s began with long durations and steadily decreased them, and a third type in which there were at first no images, then images with a gradually increasing duration.[12] Our data do not indicate the presence of these types among our *S*s. Whatever variability was present, existed throughout the series for each *S*. In her recent article, Miss Shuey reports that the average duration of the images was 3 min. 35 sec. (215 sec.).[13]

[12]G. H. Miles, The formation of projected visual images by intermittent retinal stimulation, *Brit. J. Psychol.*, 8, 1915, 93-126.

[13]Shuey, *op. cit.*, 565.

It has already been pointed out that the number of phases in the images varied with the individual series of observations. It was desirable to determine whether or not the number of phases was in any way a function of the duration of the image. In order to show the relation between these two features, Table VII was prepared. Here the medians of the number of phases, the average deviation from the median, and the range (upper and lower) are given.

TABLE VII
Total Number of Phases, Medians, Average Deviations, and Ranges of the Numbers (12 *Ss*, 12 Observations Each)

| *S* | Total Number of Phases | Median | Av. Dev. | Range | |
|---|---|---|---|---|---|
| | | | | Upper | Lower |
| By | 325 | 26.5 | 3.2 | 38 | 20 |
| Bn | 151 | 13.0 | 2.0 | 16 | 8 |
| Bz | 256 | 20.0 | 2.3 | 30 | 17 |
| Gl | 177 | 12.0 | 6.4 | 28 | 7 |
| Hn | 162 | 13.0 | 2.3 | 18 | 8 |
| Kh | 147 | 12.0 | 3.1 | 21 | 6 |
| Kr | 45 | 4.0 | .9 | 6 | 2 |
| Le | 444 | 37.5 | 3.8 | 43 | 23 |
| Mr | 452 | 36.0 | 6.1 | 55 | 28 |
| Ml | 209 | 17.0 | 2.6 | 25 | 11 |
| Rl | 109 | 8.0 | 2.6 | 15 | 5 |
| Wg | 66 | 5.5 | 1.1 | 8 | 4 |

The rank correlation between the medians in Table VI and Table VII is .62.

In the first place, the correlation between the medians is fairly close. In the main, those *Ss* who regularly had images which lasted for a relatively long time, reported the largest number of phases. In the second place it will be seen that there is some evidence of the relative consistency of reports from every *S*. The two largest average deviations are 6.4 and 6.1 respectively. These, it must be noted, are for those *Ss* (Gl and Mr) whose images were regularly unstable and vacillating. There is little or no evidence that the members of this group tended to approach group uniformity in this respect.

(5) *The Relative Rapidity of the Phase Changes.* Fechner and others have pointed out that the colors in the flight tend to appear and pass rapidly at the outset, but to become more stable as the image progresses. "Im Allgemeinen dauert, wie schon früher bemerkt, jede spätere Phase länger als die vorhergehende."[14] In the main, the results of these observations confirm those of previous observers. Taking each record on the basis of its duration in time, and dividing this by 4, it was possible to ascertain the number of phases in each quarter of the flight. Five of the *Ss* showed evidence of a decrease in the number from the first through to the last quarter. The rate of decrease was unequal from quarter to quarter, and varied with the individual. One *S* showed a slight increase from the first through the second, third, and fourth quarters. Two *Ss* showed a slight decrease from the first to the second, an increase to the third, followed by a slight drop to the fourth quarter. Two others had more rapid phase changes in the second and third quarters than in the first and fourth. One showed a gradual decline through the first three quarters and a slight increase in the fourth, while another showed a decline to the second and a gradual in-

[14]Fechner, *op. cit.*, 449.

crease through the third and fourth. Two *Ss* (Kh and Wg) regularly reported long stretches of one color (green and blue respectively). There was not a single instance of the prolonged occupation of the center of the image by the color red.

## IV. Summary of Results

(1) The after-image of a bright light contains areas which are variously colored and which exhibit color changes in the course of the duration of the image. In this investigation, only one of these areas was made the object of a series of observations to determine the characteristics of the color changes. This was the central area, corresponding in form with that of the primary stimulus.

(2) There were marked individual differences in the procession of colors in this area of the image. This conclusion holds with respect to the specific colors in the sequences, and also to the relative frequency of their appearance.

(3) As the image waned, the character of the flight of colors (procession of colors) changed. The changes were not, however, approximately identical for all *Ss*. Colors which tended to disappear from the later stages of the sequences in the images in the observations of some of the *Ss*, tended to increase in the frequency of their appearance in the corresponding stages of the images in the observations of other *Ss*.

(4) Only a few cases of 3-component recurrent color cycles were observed in this investigation. There were, however, numerous cases of 2-component recurrent color cycles. This type was called the oscillation cycle, from the fact that there seemed to be a rhythm of oscillation between two colors in the procession of colors. Within the limits of this investigation, and in the cases where this type was manifested, there was evidence of a relative constancy in the pattern of the cycle or rhythm for the individual *S* reporting them. There seemed to be, however, different patterns for different *Ss*.

(5) The duration of the flight of colors varied somewhat for different *Ss*. The number of the phases (changes within the procession or flight of colors) also varied considerably. There was a positive correlation between the duration of, and the number of phases in, the image.

(6) In the main, the colors changed more rapidly in the initial stages of the image, and later tended to change slowly.

# THE PROPERTIES OF SPACE AND TIME IN KINAESTHETIC FIELDS OF FORCE*

By C. O. Weber, Wells College

## I. The Problem and Its Import

"Whatever can be measured is real," exclaimed Max Planck; and his own triumphant success in proving the existence of quanta of electrical energy well justifies the statement. Yet there is another criterion of the real in science whose present difficulties rival the success of the principle Planck announced. This second criterion may be expressed in the phrase, "the real is the unchanging." Heretofore, this principle found perfect embodiment in the law of conservation, which Millikan regards as the 'cornerstone' of all scientific construction. It also found typical expression in the Newtonian view (which is now the traditional view of all mankind) that space and time are absolute 'reals' because they are fixed and invariable, because a unit of time and of space is always and everywhere the same. Time and space remain always and everywhere the same because *we assume that they are unaffected by the presence of physical masses or conscious observers.* Now, Einstein and his school of physical relativists assume that space and time are affected by the presence of bodies, *i.e.* gravitational fields, with the consequence that space and time cease to be absolutes. Yet, true to the instinctive human belief that the real is the unchanging, these physicists find a new absolute, namely, the velocity of light, which is the same for all observers.

From the principle that the properties of space and time depend on the presence of fields of force there flows that list of strange properties of space and time which common sense, schooled in the Newtonian tradition, finds so bizarre. Euclidean space now becomes 'real' only where gravitational influence does not reach, *i.e.* nowhere! Real space is always non-Euclidean. In a strong field of gravity space is contracted ('puckered'), time intervals are shortened, straight lines become curved, and parallel lines and degrees of angle obey new laws.[1] But reflection will show that if we consider the *kinaesthetic* perception of space, these alleged properties of space and time at once become plausible. To begin with, our muscles always perceive space *as affected by gravity*—their own weight invariably

*Read at the 35th annual meeting of the American Psychological Association, Philadelphia, December 30, 1926.

[1]See H. Poincaré, *Science and Hypothesis*, 1905, 29-32.

insures this result. Where there is intensified gravity, there is intensified 'effort,' and space should take on 'local characteristics.' We anticipate that phenomenally, Einstein's world is one in which we may feel 'kinaesthetically at home,' so to speak.

Accordingly, a new field of investigation is open to psychology, devoted to the study of the effect of fields of force on our perception of space and time. The two following experiments are a demonstration of the fact that the new physical theories of time and space permit of a psychological approach. The first is an experiment on the reproduction of distances; the second an experiment on the reproduction of time intervals.

## II. The Reproduction of Distances under Load

To determine the effects of effort[2] on the properties of space I had 5 *S*s reproduce a fixed distance of 25 cm., but under varying load. The chief instrument employed for this purpose was the kinaesthesiometer devised by Michotte.[3]

This instrument is perhaps little known in this country, but it is admirable for studying forearm movements. A horizontal beam, housed in a ball-bearing boxing, moves over a curved, graduated scale. This metal scale is placed at just that distance from the center of rotation of the beam, at which its units are at once degrees of arc and metric units. The horizontal beam carries the forearm of the subject, and movements can be made away from and towards the body. A heavy metal frame, provided with leveling screws, forms the base of the instrument. (This instrument can also be used as an ergograph.) The starting point of the movements can be altered by the use of a set screw, thus enabling the experimenter to eliminate the sense of position as a criterion. Leuba took this precaution in his experiments because he was interested in the sensory criteria involved in arm movements. We did not eliminate the criterion of initial position, nor others, such as the sound of the instruments, habits of counting, and tapping movements on the part of the *S*s. We hoped to demonstrate that all *S*s make systematic errors due to load, despite any criterion or aid used by them except vision itself.

To this apparatus I added a sliding carriage, which moved along the metal scale, and could be variously loaded. When the moving beam, carrying the forearm, moved along the scale, the carriage was smoothly pushed before it. The energy required to move it was the energy required to overcome the friction between the metal scale and the metal carriage. The law of friction involved is simple: the friction between two constant surfaces is directly proportional to the force pressing them together. Thus, to double the load on the carriage results in doubling the resistance it offers.

A Dunlap chronoscope, run from a rectifier, was so attached that the time of all movements was automatically measured. To accomplish this, electrical contacts were so attached to the carriage that at the instant the subject moved the beam against the carriage the chronoscope was set going, and stopped only when the movement of the carriage was stopped. When *S* returned the beam to the initial position, the carriage was left stationary on the scale, thus registering the distance to which *S* moved.

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[2] I employ the term 'effort' because of the brevity with which it implies the mental accompaniment of load.

[3] Sold by Leon Roland-Cordier; 30 Rue de Ligne, Héverlé-Louvain. In principle, it is like Leuba's device described in this Journal, 20, 1909, 370-373.

It must be noted, therefore, that *S* reproduced only the movement away from the body with the right forearm. The *S*s quickly adapted themselves to this *Aufgabe*, would visibly relax at the end of the out-going movement, and return in idle fashion to the point of origin.

Every *S* made 50 reproductions of the constant distance of 25°, for each of four weights: 100, 150, 200, and 250 grm. respectively. But of each set of 50 reproductions, 25 were made with an empty carriage after sensing the standard distance with the carriage loaded (the 'weight first' or W. F. series). Conversely, the remaining 25 comparison movements were made with 'weight last.' We thus got 1000 judgments in all. Chance order was followed so far as the various weights were concerned, the same weight being used not more than twice in succession. However, when working with a given weight we invariably performed with it a pair of judgments in the following time order: standard loaded, comparison unloaded; then standard unloaded, comparison loaded. Thus throughout a sitting the carriage was alternately loaded and unloaded. We thus sought to eliminate contrast effect by eliminating the *expectation* to which it is generally ascribed. The distance errors made by our *S*s (see below) fit the theory of error due to contrast, yet they could not have been due to defeated expectation. If by any chance we loaded both standard and comparison, our *S*s almost invariably showed by sign or word their awareness of broken routine.

I present below two tables which give for every *S* the average distances and durations of standard and comparison movements. Table I contains the data obtained when the standard was loaded, and Table II the data for the 'weight last' series. Distances are recorded in centimeters, and the durations in sigma. The standard distance, always 25 cm., is not given. The items under D are the comparison distances. Under T′ are the average durations of the standard movements, and under T″ the average durations of the comparison movements.

The above results show that when distances are reproduced under load, all *S*s tend to give a distance shorter than the unloaded standard, and this systematic error is proportional to the weight used. The duration of these movements (executed incidentally since the *S* has her attention on the distances) becomes longer the greater the load. If now we consider the results when the standard is loaded (Table I) and the comparison movement unloaded, the space errors become positive. The greater the load of the standard movement, the greater is the plus error of the *S* when she tries to reproduce it. However, the durations behave as they did in the previous case. That is, despite the fact that the carriage is unloaded, *S* takes more time for making a comparison movement after 'sensing' a distance under load. Although all of my *S*s were wholly untrained, their results are (with several exceptions at local points) uniformly alike.

One significant feature of the above results must be noted. We have pointed out the rule that the heavier the load on the standard movement, the greater is the plus error of the comparison movement that follows. This rule is definitely broken in Table I, in the average (for all *S*s) of the 250 grm. distance column. The average distance for this weight (26.66) is practic-

TABLE I

The Effects of Load on the Extents and Durations of Arm-Movements, Standard Loaded and Comparison Unloaded

| *S* | 100 Grm. | | | 150 Grm. | | | 200 Grm. | | | 250 Grm. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | D | T′ | T″ | D | T′ | T″ | D | T′ | T″ | D | T′ | T″ |
| Ba | 30.59 | 650 | 573 | 31.02 | 665 | 555 | 31.84 | 695 | 570 | 31.06 | 720 | 606 |
| Bo | 25.11 | 956 | 793 | 25.62 | 1033 | 851 | 25.58 | 1060 | 845 | 25.05 | 1120 | 866 |
| Ma | 25.10 | 795 | 920 | 25.28 | 898 | 961 | 25.58 | 893 | 948 | 25.76 | 928 | 961 |
| Mo | 26.93 | 693 | 580 | 26.61 | 715 | 585 | 26.16 | 725 | 583 | 26.29 | 755 | 555 |
| Tu | 25.53 | 746 | 700 | 25.54 | 801 | 716 | 25.29 | 813 | 766 | 25.15 | 768 | 743 |
| Av. | 26.65 | 768 | 713 | 26.82 | 822 | 734 | 26.89 | 837 | 742 | 26.66 | 858 | 746 |

TABLE II

The Effect of Load on the Extents and Durations of Arm-Movements, Standard Unloaded, Comparison Loaded

| *S* | 100 Grm. | | | 150 Grm. | | | 300 Grm. | | | 250 Grm. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | D | T′ | T″ | D | T′ | T″ | D | T′ | T″ | D | T′ | T″ |
| Ba | 29.63 | 565 | 616 | 29.31 | 588 | 616 | 28.88 | 603 | 646 | 28.34 | 563 | 670 |
| Bo | 24.32 | 898 | 920 | 23.56 | 928 | 1010 | 23.48 | 896 | 1001 | 23.23 | 883 | 1043 |
| Ma | 24.95 | 770 | 985 | 24.50 | 798 | 1026 | 24.43 | 803 | 1058 | 24.67 | 843 | 1058 |
| Mo | 25.38 | 623 | 621 | 25.48 | 613 | 645 | 25.38 | 661 | 733 | 25.40 | 620 | 768 |
| Tu | 24.80 | 675 | 815 | 24.74 | 683 | 831 | 24.83 | 700 | 860 | 24.85 | 598 | 871 |
| Av. | 25.82 | 706 | 791 | 25.52 | 722 | 826 | 25.40 | 733 | 860 | 25.29 | 821 | 882 |

ally identical with the equivalent figure for the 100 grm. weight (26.65). The writer believes that this is no accident. In an experiment with 7 *Ss* one year ago, with this same apparatus, we found that a load of 300 grm. was sufficient to make all *Ss* reverse their errors of distance entirely, carrying the 300 grm. load farther than the standard and unloaded movement. This reversal of error occurs at different intensities of load for different *Ss*.

### III. The Reproduction of Durations Under Load

With this same apparatus I now carried out an experiment on the reproductions of a time interval of $550\sigma$.

The procedure was as follows. *S* remained passive while 'sensing' (by hearing) this time interval, marked out by the clicks of a metronome. Then *S* sought to reproduce a time interval equal to the standard, by means of two other clicks, which she had to produce, however, by moving the carriage. *S* was thus compelled to reproduce a time interval under muscular strain or effort, which effort began and ended promptly with the reproduced time interval. *E* recorded the actual time which *S* gave as equivalent to the standard time, and also the incidental distances to which *S* moved the carriage. Five loads were used: 50, 100, 150, 200 and 250 grm. respectively. Twenty-five trials were made with every weight for every *S*. I employed 8 *Ss*, thus giving 1000 measures in all. I present the results in Table III below. Distances (D) and durations (T) again occur in terms of centimeters and sigma, and both pertain to the *S*'s reproductions.

In general, these results again show that the presence of load causes the reproduced durations to be too long, in proportion to the size of the load used. The incidental distances, on the contrary, become shorter with increasing weight. In the average results, there is only one exception to the rule that increasing load results in decreasing distance, and that occurs with the 200 grm. weight. This exception, however, is found in the individual results of 7 out of the 8 *Ss* used. An explanation may perhaps be found in the fact that this weight while moving set the carriage into a slight tremor—which was not noted and corrected until late in the experiments.

Other tentative experiments of mine point to the same central fact that fields of force produce 'local characteristics' in our perception of space and time. In one tentative experiment (now under reinvestigation) I found indications that *Ss* reproducing a standard triangle with an iron stylus over an electromagnet make their triangles too small (as measured by use of the planimeter) when the magnetic field is weak, but too large when the field is strong. This is in harmony with the results of arm-movements when a very heavy load is used. In other tentative experiments I found that *Ss*, thinking that they were drawing straight lines with an iron stylus over a magnetic field, were really drawing arcs of ellipses. When the field was weak, these elliptical arcs were drawn with the 'concave' side facing the magnetic field. However, when the field was strong, and *S* was clearly conscious of the pull of the magnet, the elliptical arc which she produced for a 'straight line' presented its convex side to the magnet, so that the magnet would fall outside of the area of the ellipse we should get if we completed the arc. This looks like a case where *S* seeks to compensate for the

TABLE III

The Effect of Load on the Reproductions of a Time Interval of 550 $\sigma$.

| S | 50 Grm. | | 100 Grm. | | 150 Grm. | | 200 Grm. | | 250 Grm. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | D | T | D | T | D | T | D | T | D | T |
| Bo | 21.50 | 881 | 19.70 | 873 | 19.02 | 1015 | 19.19 | 1068 | 18.37 | 1176 |
| Cl | 18.43 | 946 | 17.00 | 1003 | 16.84 | 998 | 16.90 | 1038 | 17.26 | 1085 |
| Ep | 23.23 | 610 | 22.25 | 625 | 20.00 | 636 | 21.00 | 630 | 21.00 | 708 |
| La | 16.86 | 888 | 16.62 | 1066 | 16.75 | 1093 | 16.34 | 1123 | 16.25 | 1163 |
| Mo | 22.74 | 1121 | 20.53 | 1156 | 20.85 | 1221 | 21.52 | 1228 | 18.97 | 1294 |
| Sh | 24.79 | 631 | 24.50 | 705 | 24.35 | 696 | 24.76 | 741 | 23.35 | 730 |
| Sp | 16.46 | 826 | 17.80 | 933 | 18.08 | 951 | 18.90 | 953 | 17.02 | 1061 |
| We | 19.50 | 451 | 17.66 | 616 | 17.58 | 728 | 16.62 | 773 | 16.41 | 868 |
| Av. | 20.44 | 794 | 19.51 | 873 | 19.18 | 917 | 19.40 | 944 | 18.58 | 1016 |

TABLE IV

Percent of V.E. for Extents and Durations of Comparison Movements, Standard Loaded, Comparison Unloaded

| S | 100 Grm. | | 150 Grm. | | 200 Grm. | | 250 Grm. | |
|---|---|---|---|---|---|---|---|---|
| | D | T″ | D | T″ | D | T″ | D | T″ |
| Ba | 8.2 | 21.2 | 7.3 | 14.9 | 8.2 | 21.5 | 9.3 | 22.3 |
| Bo | 4.8 | 8.8 | 5.1 | 9.3 | 4.4 | 11.5 | 2.4 | 8.0 |
| Ma | 6.1 | 12.3 | 4.3 | 13.3 | 6.1 | 11.3 | 6.2 | 10.7 |
| Mo | 5.5 | 14.9 | 5.2 | 19.3 | 4.5 | 21.7 | 7.4 | 18.7 |
| Tu | 5.7 | 20.9 | 4.9 | 15.4 | 3.7 | 12.4 | 4.8 | 13.7 |
| Av. | 6.06 | 15.62 | 5.36 | 14.4 | 5.38 | 15.68 | 6.02 | 14.68 |

pull of the magnet, but overdoes it. We are now engaged in a careful retrial of this experiment also, and the new results may be quite different.

## IV. Extents and Durations as Mutual Criteria in Arm-Movements

What is the underlying mechanism of these errors, and what are the sensory criteria upon which we ordinarily depend in reproducing distances and durations? Here we touch upon many vexed controversies in the history of psychology. The question regarding the sensory criteria involved has been subject to the closest scrutiny by others. We secured introspections from our *Ss* in the form of answers to questions. Because our *Ss* were wholly untrained, we do not care to offer these data for consideration, except to mention (with the greatest diffidence) several points elicited. The *Ss* were always surprised to learn that the standard remained the same distance. A standard distance executed under load seemed longer to them. They testified to a marked consciousness of strain with the 250-grm. weight. Perhaps the reversal of error characteristic of the heavy weights is marked by the advent of this consciousness of effort. Most of the *Ss* confessed to almost complete inattention to the experiment towards its end; but, curiously, this was the time when the typical errors which my tables exhibit were most nearly constant. When reproducing extents, most of the *Ss* mentioned the tendency to image the distance which they were not allowed to see. In reproducing durations, there were impulses to count, to make rhythmic movements with the idle hand or the feet, etc. For adequate studies of the sensory criteria involved, I refer the reader especially to the references below and their bibliographies.[4] In this paper, I confine myself to the point upon which my data particularly bear. This is the contention of Kramer and Moskiewicz,[5] Jaensch,[6] Leuba,[7] and others, that judgments of extent rest on the judgments of duration which

[4] A most adequate criticism of the view that sensations alone can account for space perception is W. McDougall's analysis (*Outline of Psychology*, 1923, 235-264). Cf. also E. B. Titchener, *Textbook of Psychology*, 1909, 160-182; J. Jastrow, *Mind*, 1886, 539-554; H. D. Cook, Tactual and kinaesthetic space, *Psychol. Bull.*, 12, 1915, 214-216; W. W. Costin, The influence of the variation of weight upon the judgment of extent, *Psychol. Mon.*, 10, 1909, 38-54; W. B. Pillsbury, Does the sensation of movement originate in the joint, this JOURNAL, 12, 1900, 346-353; L. B. Hoisington, On the non-visual perception of the length of lifted rods, this JOURNAL, 31, 1920, 114-146; F. Kennedy, *Psychol. Rev.*, 7, 1900, 305-309; H. K. Wolfe, Some judgments on the size of familiar objects, this JOURNAL, 9, 1897, 138-166; R. S. Woodworth, On the voluntary control of the force of movement, *Psychol. Rev.*, 8, 1901, 350-359; H. Pioncaré, *Foundations of Science*, 1921, 415 ff.; M. F. Washburn, *Movement and Mental Imagery*, 1916. Washburn's book contains analyses which are indispensable to a proper understanding of the above results, especially as regards the reversal of errors characteristic of large load.

[5] F. Kramer and G. Moskiewicz, Beiträge zur Lehre von den Lage- und Bewegungsempfindungen, *Zsch. f. Psychol.*, 25, 1901, 101-125.

[6] E. R. Jaensch, Über die Beziehungen von Zeitschätzung and Bewegungsempfindung, *ibid.*, 41, 1906, 257-279.

[7] J. H. Leuba, An apparatus for the study of kinaesthetic space perception, this JOURNAL, 20, 1909, 374-385.

accompany movements. Woodworth[8] and Hollingworth[9] dispute this result. Hollingworth concludes, from an extended experiment, that the perception of duration cannot be the basis upon which we judge distances. This is evident, for one thing, from the fact that the variable error for the durations of movements is greater than the variable error for the extents of these same movements. Judgments of extent, therefore, cannot rest on data more variable in error than they are themselves. My own computations of variable error for the arm-movements experiment agree with Hollingworth's finding that the errors (percent of variable error) are always greater for the durations than for the extents. The tables below give the average percent of V.E. for every *S*.

TABLE V

Percent of V.E. for Extents and Durations of Comparison Movements, Standard Unloaded, Comparison loaded

| | 100 Grm. | | 150 Grm. | | 200 Grm. | | 250 Grm. | |
|---|---|---|---|---|---|---|---|---|
| *S* | D | T′ | D | T′ | D | T′ | D | T′ |
| Ba | 5.3 | 21.1 | 7.2 | 19.5 | 5.9 | 21.3 | 6.0 | 19.1 |
| Bo | 3.6 | 7.8 | 5.6 | 13.5 | 3.2 | 7.4 | 4.8 | 8.6 |
| Ma | 4.1 | 12.6 | 4.1 | 11.8 | 3.4 | 9.4 | 6.2 | 15.3 |
| Mo | 5.8 | 17.8 | 5.9 | 17.7 | 4.5 | 16.2 | 4.0 | 23.0 |
| Tu | 5.0 | 19.8 | 3.4 | 13.2 | 4.3 | 11.2 | 3.2 | 12.0 |
| Av. | 4.76 | 15.82 | 5.24 | 15.14 | 4.26 | 13.10 | 4.84 | 15.60 |

In Tables IV and V, there is not a single instance in which the percent of V.E. for duration is not greater than the corresponding measure for extent. However, in computing these measures for the second experiment, where the *S* attended to durations and only incidentally reproduced extents, the results are different. In the individual results, the percent of V.E. is sometime lower for the extents, and sometimes lower for the durations. However, the average results for all *S*s show a constantly lower V.E. for the durations as compared with the extents, although only slightly lower. In Table VI we give these average results for all *S*s.

TABLE VI

Percent of V.E. for Extents and Durations when Extents are 'Incidentally' Executed: Averages for 8 *S*s

| 50 Grm. | | 100 Grm. | | 150 Grm. | | 200 Grm. | | 250 Grm. | |
|---|---|---|---|---|---|---|---|---|---|
| D | T | D | T | D | T | D | T | D | T |
| 14.2 | 13.0 | 14.0 | 12.0 | 12.2 | 11.0 | 14.6 | 11.4 | 13.7 | 11.8 |

[8] R. S. Woodworth, *Le Mouvement*, Ch. iv, 1900.

[9] H. L. Hollingworth, The inaccuracy of movement, *Arch. Psychol.*, No. 13, 1909.

But the fact remains that our *S*s (see Tables I and II) tend to reproduce the time of the standard movement, even when the comparison movement is unloaded. The V.E., after all, exhibits only the minor variations of extents and durations; but the 'bulk' of the time taken for the standard movements tends to reappear in the comparison movements, except for the disturbing effect of load. Hollingworth too obtained positive correlations between the times and the extents of movement;[10] a fact in harmony with Leuba's finding that when an *S* calls two distances 'equal,' he has executed them in the same time.[11]

We are inclined to think that the criteria which determine the quantity of the comparison movement are many, and that the time of the standard movement is one of them. Our experiments show that kinaesthetic strain or 'effort' is another important factor, as is also the velocity of the standard movement. In fact, Leuba mentions velocity as an apparent criterion, and notes, in an experiment involving muscle resistance, that there is a gradual reduction of velocity with increased resistance. This is completely verified by our results. If we divide the average time of the standard movements by their average extents, we get velocities whose constancy is greater throughout the entire experiment than that of any other figure. But even velocities show systematic errors. Increase of load invariably reduces velocity; and there is also another loss of velocity which is characteristic of the comparison movements, irrespective of load. Perhaps *S* reproduces *work*, judging the extent of a distance by the work required in traversing it.[12] If so, then the products of load times distance should be constant. Inspection of the crude data leads us to expect this, since as the load becomes greater the distances become smaller, so that the products of the two should approach constancy. Unhappily, this constant fails to appear. I do not think that my data are an adequate test either of the hypothesis that *S* reproduces *work* or of the hypothesis that she reproduces *power* (work divided by time). The 'set up' of my apparatus renders it uncertain whether the energy required to move the carriage is proportional to the weight used. This is because of the curved trajectory, because of lateral friction, and of the general crudeness of the carriage. So far as the factors which determine the judgments of space and time are concerned, I am sure that they are not wholly sensory, but that we must recognize the fundamental principle insisted upon by McDougall, that a definite 'perceptual thinking' is involved in all judgments of space and time.[13]

[10] H. L. Hollingworth, Experimental studies in judgment, *Arch. Psychol.*, No. 29, 1913.
[11] *Op. cit.*, 377.
[12] C. O. Weber, The psycho-genesis of space, *Monist*, 32, 1922, 449 ff.
[13] W. McDougall, *Outlines of Psychology*, 1923, Ch. VIII.

## Conclusions

Perhaps our general results will gain in significance if we orient them with reference to the problem of a phenomenal geometry. The field of psychology, we believe, must fundamentally remain the field of *phenomenology* from the subjective point of view. Initially, the data of the physicist and the psychologist are to a great extent identical: it is the point of view which differentiates them. The psychology here referred to, fundamentally Wundtian (and perhaps neo-Realistic as well), finds scientific 'objectivity,' not in the popular sense of an object independent of mind, which exists irrespective of its qualities. To mimic physics, the 'mother of sciences,' by finding a psychological object like a physical object is the basic error of behaviorism. Psychological data are here regarded as objective, not through being independent of all observers but through being independent of any particular observer.

Our experiments suggest the possibility of discovering a phenomenal geometry whose qualitative characteristics are objective in the above sense as independent of a particular observer, *i.e.* are the same for all observers in the same situation. The interest of the physicists themselves in such a geometry is growing.[14] We may enumerate the chief properties of such a geometry, so far as kinaesthetic space is concerned, as follows:

(1) The phenomenal geometry of the kinaesthetic sense is a geometry of space-time.

(2) Its space and time are radically altered by the presence of 'fields of force' or load. (a) A given distance under load is phenomenally equivalent to a greater distance under less load. (b) A given time interval under load is phenomenally equivalent to a smaller time interval (as measured by clock-time) under less load.

(3) The above effects of load are directly proportional to the load used; but at a critical point, increase in load tends to reverse the space errors.

(4) There are tentative results indicating that load equally effects the perception of area, and the phenomenally straight line, and therefore perhaps also degree of angle.

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[14]There is the well-known interest of Henri Poincaré, of Whitehead, Broad, and other English realists. Note Jean Nicod's *La geometrie dans le monde sensible*, Paris, 1924, especially the introduction to it by Bertrand Russell. Cf. H. Poincaré, *Dernières pensées*, 1924, ch. 2.

# THE PLEASANTNESS OF BRIGHTNESS COMBINATIONS*

By JOHN T. METCALF, University of Vermont

## I. PROBLEM

The pleasantness of color combinations has been made the subject of considerable experimental study. Relatively little attention, however, has been given to the question of possible differences in the affective character of different brightness combinations. The investigation to be reported here is a study of aesthetic reactions to certain colorless stimulus-objects, each one consisting of two shades of different intensity.

*Historical.* As far as the writer has been able to determine the only direct experimental attack upon this problem hitherto has been made by Cohn.[1] In the course of a comprehensive study of the pleasantness of single colors, brightnesses, and their combinations, this investigator devoted a section of his work to a study of the pleasantness of brightness combinations. Two color-mixers were used, on each of which were mounted two pairs of black and white disks, a large pair and a small. The disks were set each time to give the desired brightnesses in each of the two combinations which the subject was to compare with each other. The method of paired comparisons was used, 6 *Ss* taking part in one series and 4 in another. Cohn reaches the general conclusion that the pleasantness of a combination varies with the difference in brightness between the components. The combination of black and white was preferred the greatest number of times, and other combinations were preferred in proportion to the difference in brightness between their component shades. The single exception to this rule appeared in the case of one *S* who was color-blind. When single shades were compared with each other the only clear result was a preference for white.

While Cohn's investigation was in progress Major was working on a somewhat similar problem.[2] Cohn's article appeared first, and Major therefore makes, in his published report, a careful comparison between his own results and those of Cohn. In so far as they bear upon our present problem the results of the two investigators differ widely. Major's stimuli consisted of square pieces of Bradley papers, exposed singly against each of two backgrounds, black and white. Every *S* rated the affective value of each stimulus on a scale of 7 steps. Essentially, then, this is a study of the pleasantness of single shades. However, as each shade appears against a background of a different shade (black or white), and as it is found that the brightness of the background affects the judgment, the results should throw light on the question of the pleasantness of combinations. As regards the pleasantness of shades and their combinations, Major's results differ from Cohn's in two chief respects. First, there is no trace of the preference of white to gray, except an equivocal one in the results obtained from one *S*. Second, black is not pleasanter on the white background than any of the grays, except that two *Ss* rate 'green-gray' lower than black.

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*Read in part before the American Psychological Association at the Madison, Wisconsin, meeting, December, 1923.

[1] J. Cohn, Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten und ihrer Combinationen, *Philos. Stud.*, 10, 1894, 562-603.

[2] D. R. Major, On the affective tone of simple sense-impressions, this JOURNAL, 7, 1895, 57-77.

## II. Material and Method

In our own experiments the stimulus-objects were made up of square pieces of white, gray, and black paper. Every one consisted of a 3-in. square of one shade, in the center of which was mounted a 1-in. square of a different shade. For convenience in handling, the larger square was mounted on a cardboard square of exactly equal size. Five intensities of the brightness scale were used: white, black, and 3 grays, light, medium, and dark. All possible combinations of these 5 shades were used, each shade serving once as center and once as background. This gave a set of 20 stimulus-objects. The first set made up proved to be unsatisfactory for several reasons. It was made in the spring of 1923 from black, gray, and white papers then on hand in the laboratory. The various shades differed from each other in the texture of the paper; some of them were tinged with color, and the three grays were far from the intensities desired. The results of the preliminary experiments made with these stimuli are therefore not included in this report.

The second set of stimuli was made up from black, white, and gray Hering papers obtained from C. H. Stoelting Co. Grays from the 9.5 x 18 cm. sample books were used, as, in the case of the ight gray and the dark, these were found to be more satisfactory than the similarly numbered shades in the large sheets. Titchener has called attention to the defects of this series of shades, pointing out that some of the grays are tinged with color, and that some vary from standard brightness.[3] The three grays used in this experiment (Nos. 3, 9, and 36) were fairly colorless. None of the *Ss* mentioned the presence of any color as some of the *Ss* in the preliminary series with the first set of stimuli had done.

The brightness of the grays was determined by matching them with rotating black and white discs cut from the black and white papers that were used, along with the three grays, in making up the stimulus-objects. It was desired to obtain an equal-step series of brightnesses from black (B) to white (W), with the medium gray (M) just half-way between the extremes, the dark gray (D) just half-way between B and M, and the light gray (L) just half-way between M and W. The three grays were chosen to meet these requirements by two *Os* working under the same conditions of illumination as obtained in the experiment itself. The field against which they were chosen was white. After they had been so chosen the brightness of the grays was determined. In terms of degrees of white in the color-wheel, the measurements are as follows: $L = 230°$, $M = 105°$, $D = 40°$. The brightnesses of the stimulus-objects were measured again after the experiment had been completed to make sure that the grays had not faded or otherwise changed in the course of the investigation.

In the preliminary series the experiments were carried on by diffuse daylight. This, however, varied so much in intensity, even within a single experimental session, that it was decided to work by artificial light, shutting out all daylight from the room and using a 'daylight' lamp. A 100-watt National C-2 Mazda bulb was placed 1.85 m. directly above the horizontal field on which the stimuli were shown. Rich, on the basis of color equations worked out by this form of illumination and also by natural daylight, concluded "that the Mazda lamp, although dependable through the middle range of the spectrum, gives a light which is markedly deficient in blue, rays as compared with daylight."[4] Since in our experiment we are concerned with color only in the effort to prevent its appearance in the stimuli this deficiency in blue rays is not a serious matter.

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[3] E. B. Titchener, The Hering grey papers, this Journal, 25, 1914, 297-298.

[4] G. J. Rich, The daylight Mazda lamp in the psychological laboratory, this Journal, 30, 1919, 315.

Before beginning an experiment the experimenter always allowed about 5 minutes for the *S*'s eyes to become adapted to the illumination.

The field against which the stimulus-cards were displayed was white for Series I, medium gray for Series II, and black for Series III. These fields consisted of large sheets of paper 15 x 20 in., the same as that used for the corresponding shades in the stimuli themselves. A vertical screen, matching the field, hid the stimulus-cards which were not being displayed, and *E*'s records from the *S*'s view.

Throughout this report the large sheet of paper on which the stimulus-cards were shown will be called 'field.' The term 'background' will refer to the 3-in. square which forms the background of the stimulus-card. The term 'center' will refer to the 1-in. square which forms the center of the stimulus-card. For convenience the stimulus-cards were numbered, after being arranged in order from light to dark. First they were arranged into five groups according to the brightness of the background, and then the four cards of each group were arranged according to the brightness of the center. The arrangement and numbering is shown in Table I which gives the numbers of the cards and the shade of background and center in each case.

TABLE I

Showing for every Card the Shade of the Background and the Shade of the Center

| Card No. | Backgr. | Center | Card No. | Backgr. | Center | Card No. | Backgr. | Center |
|---|---|---|---|---|---|---|---|---|
| 1 | W | L | 9 | M | W | 17 | B | W |
| 2 | W | M | 10 | M | L | 18 | B | L |
| 3 | W | D | 11 | M | D | 19 | B | M |
| 4 | W | B | 12 | M | B | 20 | B | D |
| 5 | L | W | 13 | D | W | | | |
| 6 | L | M | 14 | D | L | | | |
| 7 | L | D | 15 | D | M | | | |
| 8 | L | B | 16 | D | B | | | |

The method employed was that of paired comparisons. Every card in the series of 20 was treated as a single color is treated in the usual color-preference experiment. *E* placed the two cards to be compared side by side, about an inch apart, in the middle of the horizontal field, and removed them as soon as *S* had given his response. Sometimes two sessions were required to compare every card with every other one (190 judgments). More often, however, it was possible to complete the experiment in one session. The *S* was always questioned in the course of an experimental session as to his state of fatigue, and, if his reply indicated that he was at all tired, he was allowed a brief period of rest. The writer acted as *E* throughout.

Tht *S*s were told that they would be shown a number of cards, two at a time, and that each time they were to indicate which of the two was pleasanter. In case both were unpleasant, they were to indicate the less unpleasant of the two. In case there was no difference in pleasantness between the two, they were to call them equal. They were cautioned against making the judgment on other grounds than the pleasantness or unpleasantness of the two cards actually before them. These instructions were not given *S* in a set formula, but *E* explained to each *S* what he was to do, and satisfied himself that *S* understood before beginning the experiment.

It was natural that some 'equal' judgments should appear. The differences in the affective quality of brightness combinations are probably

much smaller than similar differences in the field of color combinations. Experience showed, however, that *S* was usually able to make his judgment readily and easily in favor of one card or the other. Though all of the 24 *S*s were instructed to respond with 'equal' in case there was no difference in the pleasantness or unpleasantness of the two cards, only 9 *S*s gave 'equal' judgments. The total number of such judgments, 76, is less than 2% of the 4560 judgments made in the experiment as a whole. When 'equal' judgments did appear, they were treated in the usual way by crediting a half point to each of the cards involved.

In Series I 12 *S*s took part. Ten of these were students in the writer's classes at the University of Vermont. The other two were both college graduates. One of the student *S*s was red-green color-blind. Six *S*s took part in Series II, and six in Series III, all of them students. In each of the three series half of the *S*s were men and half were women. *E* noted down any comments made by the *S*s which had a bearing upon the preferences they had expressed, but did not try to elicit such comments by questions until the end of the experiment. After the 190 comparisons had been completed, however, *E* picked out certain of the cards which had been preferred a large or a small number of times and asked *S* to look at them and state, if possible, why he found them pleasant or unpleasant. *S* was also encouraged at this time to make any general statements or comments that he could. For the most part the *S*s were not sufficiently trained in psychological methods to be able to give very accurate introspective reports, though some of their comments taken in connection with the objective records of their preferences proved illuminating.

## III. Results

Table II gives the complete results for the three series. The numbers in the vertical columns of this table show how many times a given *S* preferred each one of the different cards. Totals and averages are given for each series separately and for the three series combined. The averages are also presented in graphic form in Figs. 1 and 2. In these figures the numbers of the cards appear along the base-line, and the height of a curve above a card's number indicates the average number of preferences for that card. In Fig. 1 the continuous line represents the averages for the three series combined, the averages being obtained by dividing the total number of preferences for a given card by 24—the number of *S*s. Since Series I includes 12 subjects and Series II and III but 6 each, Series I (W field) has double weight. If the averages for the separate series are averaged, Series I has no more weight than the other two. This procedure was also followed, and the resulting averages are represented in Fig. 1 by the dotted line. It will be observed that the two curves follow each other fairly closely, but that the dotted line lies slightly above the other for the first half of its course, and below it for the second half.

In Fig. 2 the averages for the three series are represented by the three lines. Series I (W field) is represented by the dotted line, Series II (M field) by the dashed line, and Series III (B field) by the continuous line. The three curves may be said to follow roughly the same general course. Their differences reveal an influence of field upon the pleasantness of the different cards. It will be remembered that the cards are grouped according to brightness of background, cards 1–4 having W backgrounds, cards 5–8 having L backgrounds, and so on. If the curves of Fig. 2 are studied, it appears that there is a general tendency for a card to be less preferred when its background matches the field, and more preferred when its background differs from the field. Thus, at the start where cards 1–4 have white backgrounds, the curve for the white field is lowest, and those for the gray and black fields higher. At the finish, where cards 17–20 have black backgrounds, the opposite arrangement of curves is found. This

## TABLE II

### Showing for every *S* the Number of Preferences for every Stimulus-Card

| Stimulus Cards | Series I | | | | | | | | | | | | | | Series II | | | | | | | | Series III | | | | | | | | Series I, II, III | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | SUBJECTS | | | | | | | | | | | | Total | Av. | SUBJECTS | | | | | | To-tal | Av. | SUBJECTS | | | | | | To-tal | Av. | Total | Av. |
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | | | 13 | 14 | 15 | 16 | 17 | 18 | | | 19 | 20 | 21 | 22 | 23 | 24 | | | | |
| 1 | 5 | 4 | 4 | 12 | 11 | 13 | 6 | 2 | 1 | 2 | 1 | 15.5 | 76.5 | 6.4 | 6 | 8 | 12 | 9 | 16 | 19 | 70.0 | 11.7 | 19 | 4 | 14 | 12 | 1 | 5 | 55.0 | 9.2 | 201.5 | 8.4 |
| 2 | 9 | 6 | 9 | 9.5 | 11 | 5 | 2 | 12 | 4 | 7 | 5 | 13 | 92.5 | 7.7 | 9 | 13 | 13 | 6 | 16.5 | 16 | 73.5 | 12.3 | 15 | 3.5 | 13.5 | 14 | 13 | 12 | 71.0 | 11.9 | 237.0 | 9.9 |
| 3 | 11 | 6 | 7 | 9.5 | 7 | 4 | 4 | 12 | 6 | 7 | 7 | 5 | 85.5 | 7.1 | 9 | 8 | 11 | 4 | 16.5 | 6 | 54.5 | 9.1 | 14 | 2.5 | 14 | 9 | 16 | 9 | 64.5 | 10.8 | 204.5 | 8.5 |
| 4 | 13 | 3 | 9 | 6.5 | 2 | 1 | 5 | 11 | 4 | 2 | 1.5 | .5 | 58.5 | 4.9 | 5 | 2 | 11.5 | 1 | 17 | 3 | 39.5 | 6.6 | 15 | 2 | 17 | 9.5 | 14 | 13 | 70.5 | 11.8 | 168.5 | 7.0 |
| 5 | 13 | 7 | 3 | 12.5 | 8 | 12 | 5 | 6 | 6 | 6 | 7 | 16.5 | 102.0 | 8.5 | 4 | 7 | 12.5 | 9 | 5.5 | 15 | 53.0 | 8.8 | 16 | 13.5 | 11 | 12 | 4 | 2 | 58.5 | 9.8 | 213.5 | 8.9 |
| 6 | 10 | 12 | 13 | 16.5 | 16 | 17 | 18 | 18 | 4 | 13 | 14 | 18 | 169.5 | 14.1 | 13 | 18 | 14.5 | 14 | 13 | 14 | 87.5 | 14.6 | 16 | 13.5 | 12 | 18 | 7 | 16 | 82.5 | 13.8 | 339.5 | 14.1 |
| 7 | 9 | 14 | 8 | 14.5 | 15 | 6 | 10 | 14 | 8 | 11 | 13 | 10 | 132.5 | 11.0 | 19 | 18 | 15 | 11 | 9.5 | 6 | 78.5 | 13.1 | 13 | 10.5 | 9.5 | 14 | 13 | 17 | 77.0 | 12.8 | 288.0 | 12.0 |
| 8 | 11 | 10 | 12 | 9.5 | 5 | 9 | 13 | 15 | 12 | 6 | 8.5 | 3 | 114.0 | 9.5 | 17 | 11 | 15 | 6 | 15 | 8 | 72.0 | 12.0 | 12 | 4.5 | 11.5 | 10.5 | 11 | 17 | 66.5 | 11.1 | 252.5 | 10.5 |
| 9 | 11 | 12 | 9 | 15 | 14 | 7 | 6 | 5 | 11 | 10 | 14 | 13.5 | 127.5 | 10.6 | 6 | 15 | 11 | 9 | 12 | 13 | 66.0 | 11.0 | 10 | 13.5 | 15 | 16 | 10 | 12 | 76.5 | 12.8 | 270.0 | 11.3 |
| 10 | 7 | 15 | 13 | 17 | 14 | 16 | 17 | 5 | 9 | 16 | 13 | 17 | 159.0 | 13.3 | 10 | 16 | 10.5 | 13 | 6.5 | 14 | 70.0 | 11.7 | 12 | 18.5 | 13.5 | 18 | 9 | 15 | 86.0 | 14.3 | 315.0 | 13.1 |
| 11 | 4 | 18 | 14 | 11.5 | 16 | 13 | 15 | 9 | 6.5 | 16 | 19 | 16 | 158.0 | 13.2 | 15 | 16 | 8 | 17 | 3 | 9 | 68.0 | 11.3 | 7 | 14.5 | 11.5 | 14 | 7 | 16 | 70.0 | 11.7 | 296.0 | 12.3 |
| 12 | 10 | 13 | 15 | 10.5 | 11 | 10 | 11 | 18 | 13 | 17 | 15 | 7.5 | 151.0 | 12.6 | 15 | 13 | 11 | 13 | 9 | 4 | 65.0 | 10.8 | 4 | 11.5 | 10.5 | 12 | 9 | 15 | 62.0 | 10.3 | 278.0 | 11.6 |
| 13 | 15 | 11 | 7 | 12 | 6 | 5 | 2 | 5 | 15 | 5 | 15 | 6 | 104.0 | 8.7 | 8 | 7 | 1 | 3 | 9.5 | 12 | 40.5 | 6.8 | 8 | 6.5 | 13 | 5 | 13 | 8 | 53.5 | 8.9 | 198.0 | 8.3 |
| 14 | 7 | 14 | 13 | 13 | 13 | 11 | 8 | 6 | 15 | 7 | 10 | 9 | 126.0 | 10.5 | 10 | 10 | 4 | 8 | 6.5 | 9 | 47.5 | 7.9 | 7 | 13.5 | 8 | 8 | 10 | 9 | 55.5 | 9.3 | 229.0 | 9.5 |
| 15 | 5 | 17 | 15 | 8.5 | 16 | 15 | 14 | 5 | 11 | 11 | 16 | 14 | 147.5 | 12.3 | 9 | 7 | 5 | 17 | 3.5 | 8 | 49.5 | 8.3 | 4 | 15 | 4 | 8 | 3 | 10 | 44.0 | 7.3 | 241.0 | 10.0 |
| 16 | 2 | 6 | 7 | 1 | 10 | 13 | 10 | 14 | 9 | 16 | 8 | 10 | 106.0 | 8.8 | 9 | 5 | 3 | 16 | 5 | 2 | 35.5 | 5.9 | 0 | 9.5 | 1 | 3 | 5 | 3 | 21.5 | 3.6 | 163.0 | 6.8 |
| 17 | 19 | 6 | 10 | 3 | 3 | 0 | 9 | 7 | 9 | 1 | 5.5 | .5 | 73.0 | 6.8 | 0 | 3 | 3 | 2 | 15 | 10 | 33.0 | 5.5 | 6 | 1.5 | 6 | 3 | 19 | 2 | 37.5 | 6.3 | 143.5 | 6.0 |
| 18 | 14 | 6 | 8 | 4 | 1 | 5 | 10 | 6 | 18 | 9 | 6 | 2.5 | 89.5 | 7.6 | 6 | 6 | 10 | 5 | 7.5 | 11 | 45.5 | 7.6 | 8 | 10 | 3 | 1 | 15 | 4 | 41.0 | 6.8 | 176.0 | 7.3 |
| 19 | 11 | 6 | 8 | 3 | 7 | 14 | 14 | 12 | 15 | 13 | 9.5 | 4.5 | 117.0 | 9.7 | 13 | 6 | 8.5 | 11 | 6.5 | 6 | 51.0 | 8.5 | 3 | 10.5 | 1.5 | 3 | 9 | 3 | 30.0 | 5.0 | 198.0 | 8.2 |
| 20 | 4 | 4 | 6 | 1 | 4 | 14 | 11 | 8 | 13.5 | 15 | 2 | 8 | 90.5 | 7.5 | 6 | 1 | 10.5 | 16 | 1.5 | 5 | 40.0 | 6.7 | 1 | 11.5 | .5 | 0 | 2 | 2 | 17.0 | 2.8 | 147.5 | 6.1 |

explains the course of the two curves in Fig. 1. The curve representing the averages in which white field has doubled weight lies below the other where the backgrounds are lighter, and above it where the backgrounds are darker.

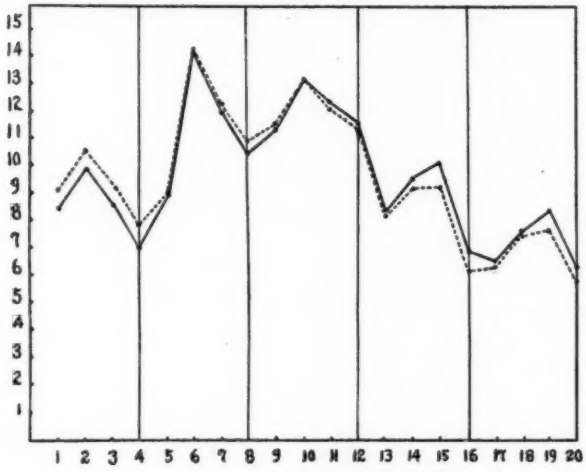


FIGURE 1

TABLE III
Showing the Comparative Pleasantness of the Different Cards

| Rank Order | Card No. | No. Prefs. | Shade of Backgr. | Shade of Center | Order in Series I | Order in Series II | Order in Series III |
|---|---|---|---|---|---|---|---|
| 1 | 6 | 339.5 | L | M | 1 | 1 | 2 |
| 2 | 10 | 315.0 | M | L | 2 | 5.5 | 1 |
| 3 | 11 | 296.0 | M | D | 3 | 7 | 7 |
| 4 | 7 | 288.0 | L | D | 6 | 2 | 3.5 |
| 5 | 12 | 278.0 | M | B | 4 | 9 | 10 |
| 6 | 9 | 270.0 | M | W | 7 | 8 | 3.5 |
| 7 | 8 | 252.5 | L | B | 10 | 4 | 8 |
| 8 | 15 | 241.0 | D | M | 5 | 13 | 15 |
| 9 | 2 | 237.0 | W | M | 14 | 3 | 5 |
| 10 | 14 | 229.0 | D | L | 8 | 14 | 12 |
| 11 | 5 | 213.5 | L | W | 13 | 11 | 11 |
| 12 | 3 | 204.5 | W | D | 17 | 10 | 9 |
| 13 | 1 | 201.5 | W | L | 19 | 5.5 | 13 |
| 14.5 | 13 | 198.0 | D | W | 12 | 16 | 14 |
| 14.5 | 19 | 198.0 | B | M | 9 | 12 | 18 |
| 16 | 18 | 176.0 | B | L | 15 | 15 | 16 |
| 17 | 4 | 168.5 | W | B | 20 | 18 | 6 |
| 18 | 16 | 163.0 | D | B | 11 | 19 | 19 |
| 19 | 20 | 147.5 | B | D | 16 | 17 | 20 |
| 20 | 17 | 143.5 | B | W | 18 | 20 | 17 |

For the purpose of studying the comparative pleasantness of the different cards, Table III has been made up from the results presented in Table II. Here the 20 cards are arranged in order from top to bottom according to the number of times each was preferred in the experiment as a whole. The card number is given in the second column, and the number of times it was preferred in the third. The next two columns indicate respectively the shade of the background and of the center of each card, and the remaining three columns give every card's rank order in the separate series.

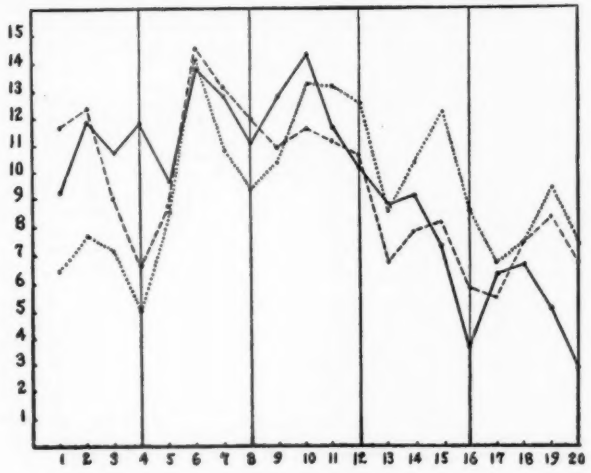


FIGURE 2

## IV. Analysis and Discussion of Results

The card most frequently preferred, No. 6, was chosen 2.4 times as often as the one preferred the smallest number of times, No. 17. If each card had been preferred an equal number of times, or if every judgment had been an 'equal' judgment, each one of the cards would have 4560/20 = 228 preferences. Our card No. 14, which ranks 10th in the series of 20, was actually preferred 229 times.

When the three series are compared with each other, the degree of difference that is revealed between the pleasantness of the different combinations is about the same for each series. The curves of Fig. 2 show this, for they follow in general much the same course, and while they reach their high and low points in somewhat different places, the degrees of difference represented are quite comparable. The cards having the highest and lowest average number of preferences for the three series differ from each other as follows:

| | Highest | Lowest |
|---|---|---|
| Series I | 14.1 | 4.9 |
| " II | 14.6 | 5.5 |
| " III | 14.3 | 2.8 |

The low points show greater differences between the separate series than the high, the chief variation being in the low point for Series III. Here card No. 20 (D on B) receives on the average only 2.8 preferences. This card rates low in each of the other series too (Ser. I, 7.5, Ser. II, 6.7), but in Series III it reaches a point lower than that reached by any card in either of the other two. Black was in general the shade least preferred, and it seemed to be especially unpleasant when combined with dark gray. Table III shows that the two cards made up of B and D rate respectively 18th and 19th in the experiment as a whole, while Table II shows that card No. 16 (B an D) has an average of only 3.6 preferences in Series III. A further factor in determining the extreme unpleasantness of card No. 20 in Series III is the fact that its background matches the black field used in this series, and, as was shown above, cards tend to rate low when their backgrounds match the field.

An examination of the fourth column of Table III, giving the shades of background and center for the 20 cards arranged in order of preference, shows that the cards with gray backgrounds tend to find their places near the top. Of the first 10 cards, all but one have gray backgrounds, while of the last 10 all but three have backgrounds of black or white. With centers there is the same tendency, though not as clearly marked. Of the first 10 seven have centers of one or another of the three shades of gray, while of the last 10 five have gray centers. Six of the 20 cards consist of a gray background combined with a gray center of a different shade. All six of these are to be found among the first 10, four of them being the first four cards in the column.

This preference for gray might be due, at least in part, to the fact that in the experiment as a whole three-fourths of the results were obtained with white or black fields, and only one-fourth with a gray field. It is necessary, therefore, to see whether such a preference holds in Series II, in which the medium gray field was used. Table III shows that of the first 10 cards in order of preference for that series, seven have gray backgrounds and seven have gray centers, while of the last 10 cards, five have gray backgrounds and five have gray centers. Of the six cards which are combinations of two shades of gray, four fall in the first 10 (two of these being first and second in order), and two fall in the second 10. Thus it appears that the preference for cards having gray backgrounds or centers or both holds in Series II with the gray field, though not to as great a degree as in the experiment as a whole. The cards which consist of combinations of black and white rate very low, both in Series II, and in the experiment as a whole. There are two such cards, No. 4 (B on W) and No. 17 (W on B). Table III shows that in the experiment as a whole No. 4 rates 17th, and No. 17 rates 20th. No. 17 rates very low in each of the three series. No. 4 rates low in Series I and II, but in Series III (B field) it rates 6th.

(*a*) *The effect of field.* Table II and Fig. 2 show the differences in the number of times the 20 cards were preferred in Series I, II, and III respectively. In Fig. 3 the curves of Fig. 2 are condensed by combining the results for cards having backgrounds of the same shade. Thus, there are five points on the base-line, the first one, W, for the general average of the four cards having W backgrounds (Nos. 1–4), the second one, L, for the cards having L backgrounds (Nos. 5–8), and so on. In this figure as in Fig. 2 the dotted line represents the results of Series I (W field), the dashed line the results of Series II (M field), and the continuous line those of Series III (B field).

It has already been noted that a field which contrasts with its background enhances the pleasantness of a card. This appears clearly in Fig. 3. The greater the difference in brightness between a given background shade and the field on which it appears, the higher is the average number of preferences, represented by the height of the curve. Where the background

shade is equally different in brightness from each of two fields the curves almost coincide. This situation occurs in three places: (1) For background L (cards 5–8), which is equally different in brightness from field W and field M; (2) for background M (cards 9–12), equally different from W and B; and (3) for background D, equally different from M and B. The rule does not hold in the first of these three instances, for if it did the curves for Series I (W field) and Series II (M field) should coincide at a point below that reached by the curve for Series III (B field). The curve for Series I is low, but that for Series II is high, even a little above that for Series III. The reason for this exception to the rule, resulting in a high point in the curve representing cards with a L background on a field of M, seems to be a very special liking for the combination of L and M. Table III shows that the card most often preferred is No. 6 which has L for background and M for center, and that the one second in order is No. 10 which has M for background and L for center. In the other two instances the rule holds perfectly.

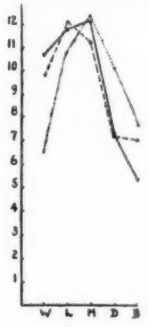


FIG. 3.

FIG. 4

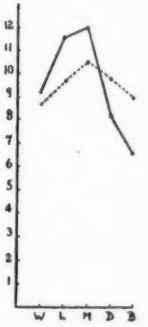


FIG. 5

The effect of the brightness of the field in determining the pleasantness of the card is not large as compared with the effect of the relationship between the two shades which form the background and center of the card itself. This may be seen from the curves of Fig. 2. These curves show less variation from each other in height at any given point than each one shows in its own height throughout its course. The widest variations of the curves from each other occur toward the ends where the white and black backgrounds are represented and where, consequently, the widest contrasts between background and field occur.

In general, then, it appears that the pleasantness of the combination depends to some extent upon the brightness relationship between its background and the field against which it is shown. The pleasantness of the combination tends to vary with the degree of difference in brightness between background and field.

Fig. 4 combines the results for cards having centers of the same shade as Fig. 3 does for those having identical backgrounds. It will be seen at once, when these two figures are compared, that the centers play a much smaller part in determining the pleasantness of the different cards than do the backgrounds, for the curves of Fig. 4 vary much less in their course than do those of Fig. 3. This is shown more clearly in Fig. 5 in which the two curves combine respectively the three curves of Fig. 3 and the three of Fig. 4. The curves for the white and gray fields in Fig. 4 follow almost

identical courses, while the black curve differs from them by running almost level. Thus there is no effect between field and center comparable to that between field and background. The black field seems to have a tendency to minimize the effect of the shade of the center and to emphasize the effect of the shade of the background. In Fig. 4 the black curve runs almost level, while in Fig. 3 it covers a greater vertical distance than either of the others.

(*b*) *Effect of relationship between center and background.* Some of the cards present combinations of shades one step apart in our brightness scale (*e.g.* W+L, D+B, M+D, etc.), others present combinations of shades two steps apart (*e.g.* W+M, L+D, etc.), still others are combinations of shades which lie three steps apart (*e.g.* W+D, L+B, etc.), and finally two of the combinations (W+B, B+W) have component shades which lie four steps apart. The cards presenting these different combinations are the following:

| | | |
|---|---|---|
| 1-step combinations | 1, 5, 6, 10, 11, 15, 16, 20 | (8) |
| 2-step " | 2, 7, 9, 12, 14, 19 | (6) |
| 3-step " | 3, 8, 13, 18 | (4) |
| 4-step " | 4, 17 | (2) |

The difference in the number of cards in which each of the different intervals is represented is, of course, due to the way in which the series was made up. One-step combinations can be made all along the line; 4-step combinations can be made only by combining the two extreme shades of white and black. In a series of 20 cards, therefore, 1-step combinations appear 8 times, and 4-step combinations only twice. This difference in number should be kept in mind in comparing the results for the different step intervals. Table IV gives the average number of preferences in the experiment as a whole for the different cards arranged according to interval. This table is derived from the last column of Table II.

TABLE IV

Showing Average Number of Preferences for the Different 'Steps'

| Card No. | 1-step | Card No. | 2-step | Card No. | 3-step | Card No. | 4-step |
|---|---|---|---|---|---|---|---|
| 1 | 8.4 | 2 | 9.9 | 3 | 8.5 | 4 | 7.0 |
| 5 | 8.9 | 7 | 12.0 | 8 | 10.5 | 17 | 6.0 |
| 6 | 14.1 | 9 | 11.3 | 13 | 8.3 | | |
| 10 | 13.1 | 12 | 11.6 | 18 | 7.3 | | |
| 11 | 12.3 | 14 | 9.5 | | | | |
| 15 | 10.0 | 19 | 8.2 | | | | |
| 16 | 6.8 | | | | | | |
| 20 | 6.1 | | | | | | |
| Av. | 10.0 | | 10.4 | | 8.7 | | 6.5 |

The averages of this table would seem to indicate that 2-step and 1-step combinations are preferred in general to 3-step ones, and these in turn to 4-step ones. It will be seen, however, from an examination of the table that there is considerable variation in degree of preference for the different cards of a given step-group. Especially is this true in the case of the 1-step combinations. Card No. 6 has an average number of preferences of 14.1, while card No. 20 has only 6.1. Thus, within this step the range of variation is practically as great as that for the table as a whole, for 14.1 is the highest value, and the only one lower than 6.1 is the 6.0 of the 4-step column. In the 1-step column the first two and the last two values are low;

the other four are high. Cards 1 and 5, the first two, both have white in the combination; cards 16 and 20 both have black. The four remaining cards are all combinations of two shades of gray. The average for the four having white or black in the combination is 7.6, that for the four gray combinations 12.4. Thus it appears that 1-step combinations rate high in pleasantness when they occur toward the center of the brightness scale, but low when they occur toward the ends of this scale. In the 2-step column the values show much smaller differences from each other, and such differences as occur do not seem to be due to the presence or absence in the combination of black or white. Only two of the six cards are combinations made up of two grays, No. 7 (D on L) and No. 14 (L on D). The remaining four involve black or white. The average for the two all-gray combinations is 10.8, that for the four others 10.3. All of the 3-step combinations necessarily involve black or white, and the two 4-step ones are, of course, combinations of black with white. In general, then, it seems that the combinations most preferred are 1-step combinations consisting of two grays. These are followed in order by 2-step combinations, 3-step combinations, 1-step combinations in which black or white appears, and finally 4-step combinations.

The results for the separate series are given in Table V in order to determine what effect, if any, brightness of field had upon the pleasantness of the different step-groups.

TABLE V

Showing Effect of Field Brightness upon the Different 'Steps'

| | 1-step (Grey) | 2-step (Grey) | 2-step (B *or* W) | 3-step | 1-step (B *or* W) | 4-step |
|---|---|---|---|---|---|---|
| Series I (Field W) | 13.2 | 10.8 | 10.2 | 8.2 | 7.8 | 5.9 |
| " II ( " M) | 11.5 | 10.5 | 10.7 | 8.9 | 8.3 | 6.1 |
| " III ( " B) | 11.8 | 11.1 | 10.0 | 9.4 | 6.9 | 9.1 |
| " I, II, III. | 12.4 | 10.8 | 10.3 | 8.7 | 7.6 | 6.5 |

In Table V the 2-step combinations as well as the 1-step ones are divided into those which are combinations of two grays and those which have in them black or white. The table shows differences between the different series in a given step group, but these are for the most part small. In only two places do they result in a change in the regular order as between step groups. The first of these is in Series II, where the gray field results in the 2-step gray combinations falling slightly below the 2-step B or W ones. The second is in Series III, where the black field has raised the relative value of the 4-step combinations (because of the effect upon card No. 4 noted above, p. 614) so that these rank higher than the 1-step B or W. Otherwise the table shows the same order for the separate series as for the combined result.

(c) *Comparative pleasantness of light center on dark background and dark center on light background.* Of the 20 stimulus-cards 10 have centers which are darker than their backgrounds and 10 have centers which are lighter than their backgrounds, each of the 5 shades having been used once as center and once as background in combination with each of the 4 others. The question naturally arises as to whether there is any preference for one arrangement or the other. Table VI gives for each of the 24 *Ss* the average number of preferences for cards in which the center is darker than its background and those in which the center is lighter than its background. The cards in which the first arrangement obtains are Nos. 1, 2, 3, 4, 6, 7, 8, 11, 12, and 16; and the cards which show the second arrangement are Nos. 5, 9, 10, 13, 14, 15, 17, 18, 19, and 20.

It will be seen from Table VI that in Series I with the white field 6 *Ss* show a preference for lighter centers, 5 for darker ones, and the remaining *S* (*S* 10) shows no preference either way. The averages for the series as a whole show 9.5 preferences for each arrangement. In the case of all except 2 *Ss* (*Ss* 8 and 9) the differences are not very marked. The results of this series, therefore, show no preference for either arrangement over the other. In Series II and Series III a preference is shown for a darker center on a lighter background. In each series 5 of the 6 *Ss* show such a preference, and the general averages are: Series II, darker center 10.8, lighter center 8.2; Series III, darker center 10.7, lighter center 8.3. The averages for all three series combined are: darker center 10.1, lighter center 8.9. In general, then, there appears to be a slight preference for darker centers on lighter backgrounds.

(*d*) *Pleasantness of combinations as related to pleasantness of single shades.* In a study of the affective tone of color-combinations, Geissler concludes that "the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination."[5] In order to determine whether preferences for combinations of shades are related to any preferences *S* may have for the component single shades, a special experiment was carried out.

Every *S*, after the experiment with the 20 cards had been completed, was given five slips of paper each of which represented one of the shades used in making up the combinations, and was asked to arrange them in the order in which he preferred them. These slips were ½ x 1½ in. in size, and were cut from the five different papers used in making up the stimulus-cards. The conditions of lighting and field were the same as had obtained in the experiment with the combinations. Table VII gives the results of this experiment with single shades for the 24 *Ss* of the three series. In the column under each *S*'s number, the number opposite the letter indicating the shade of the slip shows the position of that slip in that *S*'s arrangement. Thus, in the case of *S* 1, the black slip was placed first as most preferred of the five, the dark gray one was second, and so on. The rank orders are totalled and averaged for the separate series, and again for the three series combined.

In Series I, M is the shade most often placed first, and the average rank order is 1.8. L and D have average rank orders of 2.7 and 2.8 respectively, and W and B average rank orders of 3.9 and 3.8. Thus with these slips, representing shades chosen as 5 equal steps from W to B, M ranks first, L and D come with almost equal values next, and W and B with almost exactly equal values last. The darker fields of Series II and III result in an increase in the preference for W and L, and a decrease in that for D and B. The preference for M is very slightly reduced.

If the results just presented are compared with those given in Table III and with those represented by the different figures, it will be seen that the preference for M as a single shade is borne out by a preference for combinations in which M appears as one of the components. The columns of the table and the curves of the figures also reflect the intermediate positions of L and D, and the position at the bottom, as least preferred shades, of B and W. Thus, the pleasantness of one or both of the single shades of a combination would seem to be an important factor in determining the pleasantness of the combination.

An attempt was made to analyze this relationship more closely. Calculations were made separately for every one of the 24 *Ss* in the following manner. The *S*'s arrangement of the five slips was taken, and from it was calculated the rank order of the 20 stimulus-cards as this would have been

[5] L. R. Geissler, The affective tone of color-combinations, *Titchener Commemorative Volume*, 1917, 162.

TABLE VI
Showing the Average Number of Preferences for Cards with Centers Lighter and Darker than the Background

| Centers in Relation to Background | SERIES I SUBJECTS | | | | | | | | | | | | | SERIES II SUBJECTS | | | | | | | SERIES III SUBJECTS | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | Av. | 13 | 14 | 15 | 16 | 17 | 18 | Av. | 19 | 20 | 21 | 22 | 23 | 24 | Av. |
| Lighter | 10.6 | 9.9 | 9.2 | 8.9 | 8.6 | 9.9 | 9.6 | 6.5 | 12.3 | 9.5 | 9.8 | 9.2 | 9.5 | 7.2 | 7.8 | 7.6 | 9.3 | 7.4 | 10.3 | 8.2 | 7.5 | 11.4 | 7.6 | 7.4 | 9.4 | 6.7 | 8.3 |
| Darker | 8.4 | 9.1 | 9.8 | 10.1 | 10.4 | 9.1 | 9.4 | 12.5 | 6.7 | 9.5 | 9.2 | 9.8 | 9.5 | 11.8 | 11.2 | 11.4 | 9.7 | 11.6 | 8.7 | 10.8 | 11.5 | 7.6 | 11.4 | 11.6 | 9.6 | 12.3 | 10.7 |

TABLE VII
Showing Order of Preference for the Different Shades Used in the Experiment

| Shades | SERIES I SUBJECTS | | | | | | | | | | | | Total | Av. | SERIES II SUBJECTS | | | | | | Total | Av. | SERIES III SUBJECTS | | | | | | Total | Av. | SERIES I, II, III Grand Total | Grand Av. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | | | 13 | 14 | 15 | 16 | 17 | 18 | | | 19 | 20 | 21 | 22 | 23 | 24 | | | | |
| W | 5 | 4 | 4 | 2 | 4 | 3 | 4 | 5 | 4 | 5 | 5 | 2 | 47 | 3.9 | 4 | 3 | 3 | 4 | 2 | 1 | 17 | 2.8 | 1 | 5 | 3 | 3 | 1 | 3 | 16 | 2.7 | 80 | 3.3 |
| L | 4 | 2 | 2 | 1 | 2 | 2 | 3 | 4 | 5 | 3 | 3 | 1 | 32 | 2.7 | 2 | 2 | 2 | 3 | 1 | 2 | 12 | 2.0 | 2 | 4 | 1 | 2 | 2 | 1 | 12 | 2.0 | 56 | 2.3 |
| M | 3 | 1 | 1 | 3 | 1 | 1 | 1 | 3 | 2 | 1 | 1 | 3 | 21 | 1.8 | 1 | 1 | 1 | 1 | 5 | 3 | 12 | 2.0 | 3 | 1 | 2 | 1 | 3 | 2 | 12 | 2.0 | 45 | 1.9 |
| D | 2 | 3 | 3 | 4 | 3 | 4 | 5 | 1 | 1 | 2 | 2 | 4 | 34 | 2.8 | 3 | 4 | 5 | 2 | 4 | 4 | 22 | 3.7 | 4 | 2 | 4 | 4 | 4 | 4 | 22 | 3.7 | 78 | 3.3 |
| B | 1 | 5 | 5 | 5 | 5 | 5 | 2 | 2 | 3 | 4 | 4 | 5 | 46 | 3.8 | 5 | 5 | 4 | 5 | 3 | 5 | 27 | 4.5 | 5 | 3 | 5 | 5 | 5 | 5 | 28 | 4.6 | 101 | 4.2 |

if it had followed the rank order of the separate slips, that is, if the combinations had been ranked according to the sum of the pleasantness of the separate shades which composed them. For example, *S* No. 2 arranged the slips as follows: M L D W B, this arrangement being the most typical one. M was therefore counted as 1, L as 2, and so on. Card No. 1 consists of a W background and L center, or W+L. Substituting for the letters their numerical rank orders as determined by the arrangement of the slips, we have 4+2=6 for card No. 1. Card No. 2 is W+M, or 4+1=5. In this way values for the other cards of the 20 were calculated, and the same procedure was followed with the results of each of the other *S*s. The rank orders of the cards according to this calculation were then correlated with their rank orders as determined by the number of preferences given by *S* to each in the experiment. The formula used was $r = 1 - 6\Sigma d^2/n(n^2-1)$.

TABLE VIII

Correlation between Actual and Calculated Rank Orders

| Series I | | | | Series II | | Series III | |
|---|---|---|---|---|---|---|---|
| *S* | r | *S* | r | *S* | r | *S* | r |
| 1 | −.26 | 7 | .56 | 13 | .53 | 19 | .83 |
| 2 | .77 | 8 | .32 | 14 | .76 | 20 | .64 |
| 3 | .51 | 9 | .29 | 15 | .45 | 21 | .41 |
| 4 | .81 | 10 | .82 | 16 | .60 | 22 | .81 |
| 5 | .90 | 11 | .74 | 17 | .42 | 23 | .00 |
| 6 | .45 | 12 | .55 | 18 | .82 | 24 | .40 |
| | | Av. = .54 | | | Av. = .60 | | Av. = .52 |

Av. r, for Series I, II, III, = .55

This was done in order to get an indication of how closely *S*'s preferences for the combinations followed his liking for the separate shades of the combination. It should be recalled in this connection that background plays a more important part than center in determining *S*'s preference for a combination. This appeared in the curves of Fig. 5, and was discussed above. In the calculations just described both shades of a combination were weighted equally regardless of the fact just mentioned. If more weight had been given to the background shade the correlation would doubtlessly have been more close, but this was not done because of the lack of an exact measure of how much more important the background was than the center in determining the value of the combination.

The results of the calculations just described are presented in Table VIII, which gives opposite the number of each subject under r the coefficient of correlation between the actual and the calculated rank orders.

These r's range all the way from −.26 to +.90, but all except 4 of the 24 are +.40 or above, and 11 are +.60 or above. In the cases in which r is −.26 and .00 the *S*s, Nos. 1 and 23, were determined in their preferences solely by the degree of contrast presented. The greater the contrast the pleasanter the combination. When it was a case of arranging the single slips in order of preference, the degree of contrast between the slip and the field determined its position. Thus there was no question of preference for single shades determining preference for combinations, since in the combinations it was the degree of contrast that determined preference, and the pleasantness of the single shades themselves was relative to the background on which they appeared. *S* 17 (r=.42) and *S* 21 (r=.41) also showed a strong liking for the sharp black and white contrasts, though degree of contrast is not the only factor in determining preference, since Table II shows that some of the other combinations rate high, and Table VII shows that degree of contrast did not wholly determine the arrangement by these *S*s of the single slips. Three other *S*s have r's of less than .45.

These are *S*s 8, 9, and 24. Table VI shows that these 3 *S*s differ from the others in that they show marked preferences for the arrangement light-center-on-dark-background or for the reverse arrangement. So in the case of these *S*s too, the brightness relationship between center and background plays more of a part in determining the preference for a combination than does the pleasantness of the single shades. With the other *S*s the results seem to show that the pleasantness of the single shades is an important factor in determining the pleasantness of the combination.

(*e*) *Comments of Ss on the pleasantness of combinations.* Although, as stated above, the *S*s were not sufficiently trained in psychological methods to be able to give accurate introspective reports, their comments and explanations are of some interest when taken in connection with the objective results.

As the results of Table V show, the type of combination most preferred is a 1-step combination consisting of two grays (M+L or M+D). The comments of the *S*s on these combinations indicate two chief reasons for their pleasantness: (1) The degree of contrast is great enough to make the two shades definitely and clearly different from each other, without being so sharp as to be unpleasant. (2) The components are pleasant as single shades.

As examples of comments indicating the first reason the following may be cited:

*S* 8, Card 6. "The contrast is not too sharp, but it makes the effect clear."

*S* 20, Card 10. "Small contrast between the two grays, yet it *is* a contrast."

*S* 24, Card 11. "Seem to go together without being glaring."

A number of *S*s were evidently trying to express this factor without being able to formulate it in exact terms. The word "blend" was frequently used in this connection.

Comments indicating the operation of the second factor were less frequent, but they showed that it was recognized. For example:

*S* 11, Card 15. "I like both shades, and they go well together."

*S* 10, Card 12 (a 2-step combination). "Has as background the most attractive shade of gray."

One-step combinations involving black or white rated in general low (Table V). Card No. 1 was designated by different *S*s as "uninteresting," "insipid," "sickish," "dingy," "bleary," "vague," "not organized." Most of these comments were made when the card was displayed on the white field which matched its background. Card 20 was designated as "dismal," "not definite," and "too black." On the other hand, one *S* found it "restful." Similar comments were made with regard to cards 5 and 16.

Thus, 1-step combinations, when they are taken toward the extremes of the brightness scale, seem to the *S*s to be unpleasantly weak and lacking in character, though when they are taken toward the center of the scale they are found highly pleasant. One *S* (*S* 15), in comparing card 1 with card 7 (both, in this case, pleasant) preferred 7, giving as the reason that it was "more central." This seems to indicate an absolute preference for the middle ranges of the brightness scale, and the results of the majority of the *S*s would, of course, bear out this assumption.

Most of the *S*s, preferring in general the smaller degrees of contrast, found the 4-step combinations of B and W unpleasant. These combinations are said to have too much contrast by 18 of the 24 *S*s. The effect is variously designated as "sharp," "harsh," "too bright," "glaring," "dazzling," "fighting," "jarring," "blinding." Of the remaining 6 *S*s one (*S* 7), was non-committal as far as the black and white combinations were concerned. The results of this *S* show, however, that cards 4 and 17 both rated lower than the average. The other 5 *S*s express a liking for these

combinations. Three of the 5 *Ss* (19, 21, and 23) worked with the B field of Series III, one (*S* 1) with the W field of Series I, and one (*S* 17) with the M field of Series II. Thus, it seems likely that the B field had a tendency to increase the pleasantness of the B and W combinations. The cases of four of these *Ss* have already been considered in another connection. The other *S* (*S* 19) gives 15 preferences to card 4, but only 6 to card 17. *S* 21 also gives but 6 preferences to card 17, though giving 17 to card 4. Both state that they find pleasant the contrast of card 4 (B on W), but find card 17 (W on B) unpleasant because it is "dreary" (*S* 19), or "too black" (*S* 21). Thus these two *Ss* show only a partial preference for the B and W combinations. This leaves only three *Ss* (*Ss* 1, 17, and 23), who show very marked preference for 4-step combinations. It was pointed out above that *Ss* 1 and 23 make their choices throughout on the basis of the degree of contrast represented in the combination, but that *S* 17, while showing a preference for black-white combinations, is not determined in preference throughout by degree of contrast. In commenting on the black-white combinations, *S* 1 says that the shades "set each other off;" *S* 17 says that in contrast with each other the black and white are improved so that "each is best of its kind;" and *S* 23 says that the contrast results in a pleasing "definiteness."

The *Ss* seem to form two groups with regard to the degree of contrast they prefer. The majority form a large group showing a preference for small degrees of contrast, especially for 1-step and 2-step combinations involving two grays. There is, however, a small group consisting of three, or possibly five, *Ss* who show a preference for the sharp contrasts presented by black and white. There seems to be no intermediate group showing a preference for 3-step combinations, for there is only one *S* (*S* 9) whose results show a higher average number of preferences for the 3-step combinations than for those of any of the other step groups.

(*f*) *Sex differences.* As already stated, half of the *Ss* in each series were women and half were men. The women were Nos. 6, 7, 8, 9, 10, 12, 15, 16, 17, 19, 20, and 22; the men, Nos. 1, 2, 3, 4, 5, 11, 13, 14, 21, 23, and 24. In the study of each one of the points taken up above, calculations were made for the men and women separately, in order to determine any possible sex differences which might be revealed. The only difference found which is large enough to be at all significant is a slightly greater tendency on the part of the men to prefer contrast. The two *Ss* 1 and 23, who made their choices consistently on the basis of degree of contrast, are both men. Of the five *Ss* who showed a preference for 4-step combinations, three are men and two are women. The difference appears also in a comparison of the average number of preferences for the combinations of the different step groups given by the men and women respectively. These are as follows: 1-step: Men 9, Women 10.9; 2-step: M. 11, W. 10; 3-step: M. 9.5, W. 7.9; 4-step: M. 7.3, W. 5.8. These differences, small and probably of little significance, were the only sex differences found.

## V. Conclusion and Summary

The most important result of this study seems to the writer to be the fact that the majority of the *Ss* are found to prefer relatively small degrees of contrast. Cohn's principle that combinations are pleasanter the greater the difference in brightness between the components is consistently borne out by the results of only 2 of our 24 *Ss*. Only 5 of the 24 showed a preference for maximum contrasts. Cohn related this brightness principle to his results for color. Finding that combinations between complementary colors were preferred to other color-combinations, he formulated the

general principle: "Auf dem Gebiete der Gesichtsempfindungen wirkt die Verschiedenheit benachbarter Empfindungen lusterregend um so mehr, je grösser sie ist."[6]

Cohn's conclusion with regard to complementaries has not been borne out by subsequent investigations. Baker, in each of two separate studies on color-combinations, found very little, if any, justification for it.[7] Barber, in an extended experimental study of binary combinations in which tints and shades were used as well as saturated colors, got results which tend to confirm Baker's as far as the pleasantness of complementaries is concerned.[8] Geissler found no evidence in his results for the supposed superior pleasantness of complementaries, or even of "near-complementaries."[9] It would seem from our results that a similar revision is required for the idea that two brightnesses in combination are more pleasant the greater their difference.

Cohn's color-blind *S*, a case of red-green color-blindness, was the single exception to his rule. The red-green color-blind *S* in our experiments (*S* 6) seems to be highly sensitive to contrast, and to show a more than average preference for 1-step combinations and aversion to 4-step ones. Her average is 14.1 for 1-step combinations as compared with 10.0 for all *S*s and 0.5 for 4-step ones as compared with 6.5 for all. One would naturally expect such results from a *S* who was, as color-blind persons presumably are, especially sensitive to brightness differences. However, similar results are found for *S* 12 (14.4 for 1-step, 0.5 for 4-step combinations) whose color-vision *E* had no reason to suspect of being abnormal. It seems doubtful, therefore, that the factor of color-blindness affected the result.

Briefly summarized, the chief results of our investigation are as follows:

(1) The *S*s in general preferred combinations of two grays which represented a relatively small degree of brightness difference. Combinations of gray with black or with white were in general less preferred, and combinations of black with white least preferred.

(2) Two of the 24 *S*s found brightness combinations pleasant in proportion to the degree of brightness difference between the components. In addition, 3 *S*s also showed a preference for the black-white combinations.

(3) With the stimuli used in our experiment, background played more of a part than center in determining the pleasantness of a combination.

(4) The brightness of the field on which the stimuli were shown had an effect upon their pleasantness, increasing it when field contrasted with background, decreasing it when field matched background.

(5) The arrangement in which a dark center appears upon a light background was slightly preferred to the reverse arrangement.

(6) When single shades were arranged in the order of their pleasantness, the medium gray was in general most preferred, and was followed in order by light gray, dark gray or white, and finally black.

(7) The pleasantness of the single component shades is an important —possibly the most important—factor in determining the pleasantness of a combination.

(8) No sex differences appeared, except that the tendency to prefer small to greater degrees of contrast was slightly more marked in the results of the women than in those of the men.

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[6]Cohn, *op. cit.*, 603.

[7]E. S. Baker, Experiments on the aesthetics of light and color: I. On combinations of two colors, *Univ. Toronto Studies, Psychol. Ser.*, 1, 1900, 65 ff. (241 ff.); II. Spectrally pure colors in binary combinations, *ibid.*, 2, 1907, 42 f.

[8]F. L. Barber, Color aesthetics: II. Combinations of colors, tints and shades, *ibid.*, 2, 1907, 45 (289).

[9]L. R. Geissler, *op. cit.*, 165.

# THE FUSION OF NON-MUSICAL INTERVALS*

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Psychologically fusion is an equivocal term, and even when its meaning is carefully narrowed it corresponds to a very difficult *Aufgabe*.[1] There is also some question as to the exact meaning of musical interval.

## I. Questions of Interval

There are two ways in which a musical interval can be defined: (1) an interval in which the vibration-frequencies are in simple mathematical ratio (the simple interval of the physicist); (2) an interval which has been used in music. If we adopt the physical definition, we are confronted with the fact that the intervals on the 'tempered' scale of to-day are not in exact ratios at all. On the other hand, there are some 154 different intervals which have been used, and which, therefore, could be called musical.[2]

*Nature of Interval.* The physical nature of the interval is worthy of consideration. In the first place, the fact that the partials of a tone are in simple vibration-ratio with the fundamental and with each other gives the 'simple interval' a basis in natural experience. Moreover the facts that beats occur between two tones a short distance apart on the scale and between mistuned simple intervals (as Krüger has shown),[3] and that extraneous difference tones occur elsewhere (generally where the ratio is other than simple) tend to make the simple interval comparatively a simple sound. Thus we have two factors, one experiential and the other immediate, determining qualitative aspects of the sound which are dependent on vibration-ratio.

*Musical Scales.* As musical expression developed and there came into use more intervals than the octave and the simplest intervals, it became desirable to place the tones along a scale so that a series of intervals would be found around each tone on the scale. The modern scale of twelve semitones within the octave has been developed in the following ways:

(1) The Pythagorean scale is constructed from a series of perfect fifths which, when reduced to the compass of an octave, gives the complete diatonic scale.

(2) The just scale ascends by fifths and thirds, thus giving the third an exact ratio which it does not have in the Pythagorean scale.

(3) The meantone scale gives the fifth the ratio 109: 163, and ascends by the Pythagorean method.

(4) The tempered scale divides the octave into twelve geometrically equal parts along a scale of 1200 'cents.'

This makes it evident that it has been found impossible to devise a scale in which all tones are in simple ratio to all the others. On the Pythagorean scale the third comes out 64:81; the meantone scale, to avoid this ratio, mistunes the fifth; the just scale tunes both the third and the fifth at the expense of the other intervals; while the tempered scale mistunes them all in order to approximate all and obtain a simpler series.

The intervals c:c## and c:d are alike only on the mistuned tempered scale. On the meantone scale the value of the first is 152 cents and of the latter 193 cents. Hence we should have a far from musical interval if we had c and c## in the same chord; but, since these occur in wholly unre-

*Communicated by C. C. Pratt.

[1] C. C. Pratt, this Journal, 32, 1921, 490-515.

[2] H. Helmholtz, *On the Sensations of Tone*, tr. A. J. Ellis, 4th ed., 1912, 516.

[3] F. Krüger, *Psychol. Stud.*, 5, 1910, 294-409.

lated keys, they would practically never be found together. Slight changes of pitch of this sort are met with in enharmonic musical changes; many instrumentalists and at least some choralists are sensitive to such changes.

Melodic intervals seem to be learned rather than natural. Those who have learned to sing to the piano use the tempered intervals; some *a cappella* choralists sing the just intervals. The best instrumentalists, however, play the third as 4:5,[4] and unpracticed singers frequently use mean-tone intervals, presumably because the thirds and fifths and their inversions are pure. Artistic singers, however, vary greatly in pitch while singing a single note,[5] and naïve singers have been found to vary in the rendition of the tones of a folk-song from 76 cents (for the octave) up to 272 cents for the second.[6] So, after all, it is probably only in harmonic music that accurate intervals are used.

*Non-Musical Intervals.* We have seen how the transition from musical to non-musical intervals may take place in our present musical system. Other systems of music use definite 'non-musical' intervals. A few examples will suffice:

(a) Javanese and other oriental musics have a pentatonic scale in which the octave is divided into five parts. The divisions are equal; thus the intervals are 240, 480, 720, and 960 cents.[7]

(b) In the ninth century Zalzal in Arabia devised a three-quarter tone scale, employing intervals of 355 and 853 cents.[8] In the nineteenth century Meshaqah in Arabia used a quarter-tone scale giving many secondary (melodic) notes.[9]

(c) The Scotch bagpipe uses intervals of 341 and 853 cents.[10]

(d) An ultra-modern Hungarian, Mr. Hába, has made a quarter-tone piano and composes for it.[11] He, as is not the case in the first three examples, frankly uses 'non-musical' intervals in harmony as well as in melodic line.

*Conclusions.*[12] (1) No musical scale can be constructed which has all tones in simple ratio to all the others; some or all of the intervals must be approximated.

(2) There seems to be a phenomenological basis of recognition of pure intervals.

(3) A singer tends to sing melodically in pure intervals, but also in intervals on a scale which he has learned.

(4) He tends to sing harmonically in pure intervals, for in reproducing a tone with distraction he tends to sing tones which are harmonious with the distracting tone.[12]

(5) Non-musical intervals seem to have been used up to the present time only in homophonic music that lacks tonality or harmony.[13]

## II. Questions of Fusion

In general psychologists have been content to describe fusion not too definitely: as a product of an act of the perceptive apparatus or as a symptom of its posture. Recent accounts, especially those appearing in the *Gestalt* literature like Wertheimer's article on configurations in the visual

[4]Helmholtz, *op. cit.*, 235.
[5]M. Schoen, *Mus. Quart.*, 12, 1926, 290 ff.
[6]O. Abraham, *Psychol. Forsch.*, 4, 1923, 1-22.
[7]Ellis, in Helmholtz, *op. cit.*, 518, 522.
[8]*Ibid.*, 516-520.
[9]*Ibid.*, 525.
[10]*Ibid.*, 515. E. Smith, *J. Amer. Or. Soc.*, 1, 1847, 73 f.
[11]A. Welleck, *Mus. Quart.*, 12, 1926, 231 f.
[12]E. H. Cameron, *Psychol. Monog.*, No. 34, 1907, 227-300.
[13]Ellis, *op. cit.*, 526 f.

field,[14] have tended to bring these matters down to the level of clear explanation used heretofore on the simple phenomena of sensation.

*Tonal Fusion.* Simultaneous fusion of tones, as the simplest and most easily controlled case of fusion, has been the subject of numerous theories, and yet we can still appreciate the difficulty noted by Mach, when he said, "the lack of a positive factor in the explanation of consonance has been very generally felt."[15] It is true that every theory of tonal fusion has in general made fusion dependent upon one, and only one, 'positive factor.' Existing theories have been classified below according to their reference to these factors.

*Theories.*

*I.* There are theories which make fusion dependent on *vibration ratio.* Until very recently all theories postulated such a dependence and made the explanation of this dependence their basis.

(A) Fusion depends on *vibration ratio by means of extrinsic labels.*

(1) Fusion depends upon the partial tones—the original physical theory of Helmholtz.[16] That beating partials operate to reduce the degree of fusion is difficult to deny, but Stumpf and more recent critics have pointed out that the fact of mutual reinforcement of partial tones affords no positive explanation of the fact of fusion.

(2) Fusion depends upon the difference tones—Krüger's supplement to the Helmholtz theory.[17] This view is open to the same criticism as its predecessor, and furthermore Stumpf in refutation has shown how this theory would presuppose the fusion of a few intervals such as 8:11, 10:17![18]

(B) Fusion depends on *vibration ratio by means of immanent factors.*

(3) Fusion is a direct phenomenon of vibration ratio due to 'micropsychic' rhythmic coincidence of beats. This theory of Lipps,[19] elaborated from the speculations of Leibniz and Euler, has been refuted largely by subsequent experimental work.[20] It is incapable of explaining, for example, the good fusion of a slightly mistuned octave.

(4) Fusion depends upon a relation in the tonic (the common fundamental of two tones) or in the phonic (the common overtone). Von Oettingen made this relation a remembrance;[21] Mach placed it on the sensory level.[22] However, these theories treat of matters of quality rather than of qualitative fusion.

(5) Fusion depends upon a relational element of specific synergy in consciousness. Stumpf identified fusion with a unitariness,[23] which depends upon a similarity of a hypothetical sort not the same as similarity seen in sequence.[24] Revész found qualitative similarity only between tones in the octave relation.[25]

(6) Fusion depends upon a relational effect through association of tonal excitation. Ebbinghaus' theory is parallel to that of Stumpf, but makes the relation peripheral rather than central.[26] Stumpf considered this conception less adequate because it does not explain degrees of fusion in imagination.

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[14]M. Wertheimer, *Psychol. Forsch.*, 4, 1923, 301-350.
[15]E. Mach, *Analysis of Sensations*, tr. C. M. Williams, 1914.
[16]Helmholtz, *op. cit.*, 182 ff.
[17]Krüger, *loc. cit.*
[18]C. Stumpf, *Zsch. f. Psychol.*, 59, 1911, 161-175.
[19]T. Lipps, *Psychologische Studien*, 1905, 115 f.
[20]R. M. Ogden, *Hearing*, 1924, 133 f.
[21]A. v. Oettingen, *Harmoniesystem*, 30 ff.
[22]Mach, *op. cit.*, 224.
[23]C. Stumpf, *Abhandl. d. Münch. Akad.*, 1897.
[24]Stumpf, *Beiträge zur Akustik*, I, 1898, 50.
[25]G. Revész, *Zur Grundlegung der Tonpsychologie*, 1913.
[26]H. Ebbinghaus, *Grundzüge der Psychologie*, 1923, I, 350 f.

(7) Fusion depends upon a balance or symmetry in the excited organ. Watt based this conception upon his theory of tonal mass, and made fusion a matter of symmetrical coalescence of volumic masses.[27] There arises, however, the objection that geometrical symmetry on the basilar membrane does not evidently connote fusional phenomena, that the configurational relation is not apparent. Work on volume by Rich[28] has been used by Ogden[29] to show that the volumes of the partials cannot possibly have the proportional values demanded by Watt's theory.

*II.* There are theories which make fusion depend upon *frequency of association* or otherwise upon experiential data. A few more recent theories would make fusion wholly a matter of familiarity.

(8) Fusion depends upon familiarity established in the past experience of the individual. This view appears in some of the theories mentioned above, notably those of Von Oettingen and Ebbinghaus.

(9) Fusion depends upon familiarity in racial experience. Ogden[30] put forward frequency of association as a factor which was denied by Stumpf[31] but reaffirmed by the experimental work of Valentine[32] and Moore.[33] He admitted the additional influence of immanent and extrinsic factors.

(10) Fusion depends upon tradition and experience. In his criticism of Ogden, Peterson points out the danger of postulating an acquired somatic character which is transmitted, and appeals to tradition as the medium of transmission.[34]

As a transition to the final group of theories, it will be well to recall the work of Edmonds and Smith,[35] showing that each interval is recognizable by means of its phenomenal characteristics, described in terms of a number of factors.

*III.* There are theories which take into account *multiple factors.*

(11) Fusion in the sense of unitariness is one of a number of factors. Kemp found in the tonal complex a factor of fusion as an unalterable peculiarity of the sensory material, and also a number of other sensory and affective factors.[36]

(12) Fusion in the sense of unitariness is an integration of factors. Pratt, in the study of dimensional factors such as roughness, analyzability, disparateness, and richness, found that intervals have a different value in each dimension,[37] and seemed further to find that the results for these four factors average to the equivalent of results from unsophisticated judgments on unitariness, a conclusion which would make unitariness the integrative result of multiple factors.

## III. Experimental Investigation

The experiments described in this paper were carried on during the two academic years 1924-1926. During the first year some preliminary work was completed for a set of non-musical intervals, which were presented on an Appunn tonometer, a reed instrument giving a rich set of overtones with each stimulus. The *Os* were: Mrs. E. C. Gilbert (G); Miss H. F. Moran (M); Mr. Herbert Gurnee (HG); and Mr. Morgan Upton (U).

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[27] H. J. Watt, *Psychology of Sound*, 1917, 3 ff.

[28] G. J. Rich, this Journal, 30, 1919, 121-164.

[29] Ogden, *op. cit.*, 133 f.

[30] *Ibid.*, 140-149.

[31] Stumpf, *Tonpsychologie*, II, 1890, 208.

[32] C. W. Valentine, *Brit. J. Psychol.*, 7, 1914, 118-135.

[33] H. T. Moore, *Psychol. Monog.*, 17, 1911, No. 73, 1-68.

[34] J. Peterson, *Psychol. Rev.*, 32, 1925, 17-34.

[35] E. M. Edmonds and M. E. Smith, this Journal, 34, 1923, 287-291.

[36] W. Kemp, *Arch. f. d. ges. Psychol.*, 29, 1913, 139-181.

[37] Pratt, *loc. cit.*

The main part of the experiment was performed during the following year; musical and non-musical intervals alike were presented, on a set of Stern variators.[38] These instruments give comparatively pure tones.

For this part of the experiment the *O*s were: Dr. M. B. Pratt (MP); Mr. A. G. Ekdahl (E); Dr. F. A. Pattie (P); and Mr. E. G. Wever (W), all members of the Harvard Psychological Laboratory.

For the purposes of the experiment we took as musical intervals the 12 intervals between the octave as found on the tempered scale (in general reduced to simple vibration-ratios), and used for non-musical intervals 12 intervals so chosen that each one, as a ratio, was the geometrical mean between two of the musical intervals, *i.e.* at intervals of 50, 150, 250 cents, etc.

The objects of this experiment were in general as follows: (1) to study musical and non-musical intervals alike with reference to their relative degrees of fusion; (2) to investigate the relation between fusion and simple vibration-ratio; (3) to investigate the relation between fusion and pleasantness; (4) to investigate the relation between fusion and familiarity; and (5) to study the relation between fusion and various phenomenal aspects of the interval.

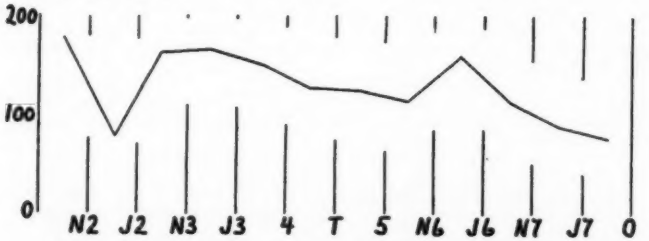


FIG. 1. NON-MUSICAL INTERVALS IN PAIRED COMPARISONS

The musical intervals of which these are the intermediates are shown as abscissae; the intervals grow larger reading from left to right. The numbers of judgments favoring each interval as more unitary is indicated as ordinates. This curve shows the combined totals for all observers. N = minor; J = major; T = tritone; O = octave.

*Preliminary Series.* In this series 12 intervals were used, and they were presented for judgment by the method of paired comparisons. At no time were any musical intervals used in this series. The order of presentation was reversed in successive series to avoid the possibility of stimulus-error through cadential effect (Kemp's factor of need of resolution).[39]

A first series was given in an attempt to fix a criterion of fusion for each *O*, preferably alike for all. They were instructed to try to form a good criterion based on the conception of the meaning of 'fusion.' Upon demanding introspections for these naïve judgments, it was quite evident that 'fusion' bore a wholly different meaning for every *O*. To one it meant *intimacy-separateness;* to another, *smoothness;* to another, *pleasantness;* while to the remaining *O* it was "too much of an intellectual conception" for easy judgments.

It was at once decided that a stricter criterion should be used, and Stumpfian 'fusion'—*unitariness*—was chosen. Six complete series of paired comparisons were given.

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[38]These variators were secured through a grant from the Milton Research Fund of Harvard University.

[39]Kemp, *loc. cit.*

It was possible to draw the following conclusions from the numerical results:

(1) Correlations between rank orders for the different *O*s show only one convincing correlation – a coefficient of 0.65 between HG and M. The others were all less than 2.5 times the probable error (see Table I).

(2) There was one very equivocal interval – the quarter tone. It was phenomenally a single tone with 8 regular oscillations of intensity (beats) per second. Two *O*s ranked it first, disregarding the beats; the others gave it positions 5 and 11.

TABLE I

I. Correlations between rank orders for each pair of observers: Unitariness series.*

| | | |
|---|---|---|
| HG–U | −0.06 | |
| U–M | 0.17 | |
| U–G | 0.22 | Probable error, 0.124 |
| HG–G | 0.29 | |
| G–M | 0.31 | |
| HG–M | 0.65 | |

II. Correlations between rank orders for each pair of observers: Pleasantness series.*

| | | |
|---|---|---|
| M–U | −0.23 | |
| M–G | 0.02 | |
| M–HG | 0.07 | Probable error, 0.124 |
| G–HG | 0.50 | |
| U–HG | 0.55 | |
| U–G | 0.66 | |

III. Correlations between rank orders for each observer, for the two series: Pleasantness and Unitariness.*

| | | |
|---|---|---|
| HG | −0.24 | |
| U | 0.14 | |
| G | 0.58 | Probable error, 0.124 |
| M | 0.73 | |
| Combined | 0.73 | |

*All correlations by Spearman's formula.

(3) There was a general trend favoring 5 of the intervals as well fused: 4 intervals in the region between the major second and the tritone, and one between the sixths.

(4) The other intervals were all very rough, being near unison, the octave, or the fifth. The greatest disagreement among the *O*s appeared for these intervals.

(5) M mentioned in introspection the 'cadential effect' and gave frequent reversals of judgment on reversal of the orders of pairs in the series.

(6) The *O*s were fairly consistent with their own results; different series of the same *O* correlate highly.

(7) Introspections were demanded on the determinants of unitariness. No leading descriptive words were suggested; hence it is significant that some *O*s used nearly the same words. (a) HG and U used as the key-word "separability," apparently some active process. (b) G and M used "apartness," apparently a passive phenomenon stated in or translated into spatial terms. (c) Most significant of all was the fact that in no case where two *O*s used the same words in describing the process did they show

any sort of correlation of rank-orders. Hence the phenomenon of unitariness would seem to be complex below the level of introspective analysis which the *Os* were using.

*The Affective Factor.* To determine the part played by affection in the perception of fusion, a series of the same proportions and using the same stimuli was presented, with instructions to judge on pleasantness-unpleasantness, to forget the old *Aufgabe* as much as possible, and to assume a more passive attitude. We may summarize the results:

(1) All *Os* but U soon lost the old *Aufgabe*; U thought the two correlated. (No such correlation, however, appears in his results.)

(2) M gave zero correlations with the other *Os* (with respect to rank-orders). The other *Os* gave high rank-order correlations (between 0.50 and 0.66) with each other.

(3) For HG and U the pleasantness series showed no correlation with the unitariness series which could be considered significant; but G and M gave a high correlation.

The highest correlation of unitariness with pleasantness was given by M, whose results for pleasantness did not correlate at all with those of the other *Os*. This difference seems to justify the conclusion that pleasantness *meant* fusion to her, and that fusion was the immediate fact. Excluding this case, our observations lead to the following conclusions:

(1) Pleasantness, far from being dependent on fusion, seems to be a more univocal datum, for in respect of pleasantness the *Os* are in agreement.

(2) Pleasantness is frequently wholly unrelated to fusion.

*Main Series. Stimuli.* The preliminary series indicated that some of the non-musical intervals might be as well fused as certain of the musical intervals. Stumpf mentioned five grades of fusion with intervals of simple ratio: (1) the octave; (2) the fifth; (3) the fourth; (4) the thirds and sixths; (5) all other (dissonant) intervals.[40] It is evident that there are several grades within Stumpf's fifth class. Is it not possible that some of the non-musical intervals among the thirds and sixths may fall in the fourth class? The judgments of a set of naïve *Os* on a series of 12 musical and 12 non-musical intervals, recorded very roughly, seemed to lend justification to some such notion.

We continued to use the Stern variators in presenting the stimuli. Their tones are practically pure; only the first overtone is at all discernible; a mistuned octave beats, but a mistuned fifth beats only faintly and other slightly mistuned intervals not at all. In all there were four variators, with ranges of 200-400, 300-600, 400-800, and 500-1000 d. v., respectively. They had some disadvantages: they were not entirely accurate, and had to be adjusted at first and calibrated against each other afterwards; and among the lower tones there was a certain hissing or 'breathiness.'

Of the *Os*, MP was the only one that might be considered particularly musical. She recognizes intervals and has considerable technical knowledge of music. The others were essentially non-musical, and P ranked particularly low in the Seashore tests. MP tried to maintain a naïve psychological attitude.

After a preliminary work-out, three series were given, with stimuli and instructions changed for each series.

*First Series.* The entire series of 24 intervals, from a quarter-tone above unison up to the octave, was presented 10 times for absolute judgments on a scale of five points. Each interval was presented 5 times in a low range (from 250 to 550 d. v.) and 5 times in a high range (from 350 to 700 d. v.) to avoid any stimulus-error due to 'breathiness' in the lower range

[40] Stumpf, *op. cit.*, 135.

or sharpness in the higher. The *Aufgabe* was first given as 'unitariness or fusion,' but the latter word was subsequently struck out because, as in the preliminary series, it bore more than one connotation.

TABLE II

DEGREES OF FUSION FOR MUSICAL AND NON-MUSICAL INTERVALS. 4 OBSERVERS

| Interval | Observers MP | E | W | P | Average |
|---|---|---|---|---|---|
| **First Series: average of ten trials** | | | | | |
| Quartertone | 3.4 | 1.6 | 4.4 | 4.8 | 3.550 |
| Min. 2d | 2.5 | 1.0 | 3.5 | 3.8 | 2.700 |
| + Min. 2d | 2.5 | 1.0 | 3.2 | 2.8 | 2.375 |
| Maj. 2d | 2.4 | 1.1 | 2.2 | 3.6 | 2.325 |
| + Maj. 2d | 3.3 | 1.5 | 3.2 | 3.2 | 2.800 |
| Min. 3d | 3.9 | 2.4 | 3.6 | 3.0 | 3.225 |
| + Min. 3d | 4.2 | 2.2 | 3.3 | 3.2 | 3.225 |
| Maj. 3d | 3.8 | 2.4 | 3.8 | 3.7 | 3.425 |
| + Maj. 3d | 2.4 | 3.2 | 3.8 | 3.5 | 3.225 |
| Fourth | 3.0 | 3.0 | 3.6 | 3.6 | 3.300 |
| + Fourth | 2.7 | 3.2 | 3.6 | 4.2 | 3.425 |
| Tritone | 3.3 | 3.7 | 3.1 | 3.5 | 3.400 |
| + Tritone | 1.7 | 2.9 | 3.9 | 2.7 | 2.800 |
| Fifth | 1.9 | 3.4 | 3.5 | 3.9 | 3.175 |
| + Fifth | 2.2 | 2.8 | 2.6 | 3.3 | 2.725 |
| Min. 6th | 3.1 | 3.7 | 3.6 | 3.4 | 3.425 |
| + Min. 6th | 3.6 | 3.3 | 3.1 | 3.5 | 3.375 |
| Maj. 6th | 3.7 | 3.1 | 3.2 | 3.4 | 3.350 |
| + Maj. 6th | 2.1 | 2.7 | 3.1 | 2.7 | 2.650 |
| Min. 7th | 2.6 | 3.3 | 3.3 | 2.8 | 3.000 |
| + Min. 7th | 2.2 | 2.4 | 2.2 | 3.2 | 2.500 |
| Maj. 7th | 1.6 | 2.3 | 2.6 | 3.0 | 2.375 |
| + Maj. 7th | 4.2 | 3.2 | 3.8 | 2.9 | 3.525 |
| Octave | 5.0 | 4.7 | 4.6 | 4.1 | 4.600 |
| **Second Series: average of six trials** | | | | | |
| Min. 3d | 3.7 | 3.0 | 2.8 | 1.8 | 2.833 |
| + Min. 3d | 3.8 | 2.7 | 3.7 | 2.2 | 3.083 |
| Maj. 3d | 4.0 | 2.8 | 3.3 | 2.3 | 3.250 |
| + Maj. 3d | 3.0 | 2.7 | 3.3 | 2.8 | 2.958 |
| Fourth | 2.2 | 4.2 | 3.7 | 2.5 | 3.125 |
| + Fourth | 1.5 | 3.8 | 4.0 | 2.8 | 3.042 |
| Tritone | 2.2 | 4.0 | 3.5 | 3.3 | 3.250 |
| +Tritone | 1.7 | 3.5 | 3.3 | 3.5 | 3.000 |
| Fifth | 1.8 | 2.7 | 4.2 | 4.3 | 3.250 |
| + Fifth | 1.7 | 3.8 | 3.7 | 3.5 | 3.167 |
| Min. 6th | 3.2 | 3.2 | 4.2 | 3.8 | 3.583 |
| + Min. 6th | 3.7 | 3.7 | 3.0 | 3.7 | 3.500 |
| Maj. 6th | 3.7 | 2.7 | 3.2 | 3.7 | 3.292 |

*Second Series.* The same instructions were given, and 13 intervals from the more equivocal part of the scale were presented each 6 times. All the musical and non-musical intervals from the minor third up to the major sixth were used; the same precaution was taken to present half in the lower range and half in the upper. Occasional judgments of the four adjacent intervals were taken.

*Third Series.* Eight intervals, 4 musical and 4 non-musical, were chosen in the central part of the scale, from the perfect fourth up to a quarter-tone above the minor sixth. These were presented for paired comparisons, and four complete series of 28 comparisons were taken. The order of presentation of each separate pair was reversed with each trial, and after the second set all the high tones were reset to become low, and *vice versa.*

The musical intervals in the first and third series were in *simple vibration-ratio*; in the second series the *tempered scale* was used. The order of presentation in every case was entirely random.

TABLE III.

DEGREES OF FUSION FOR MUSICAL AND NON-MUSICAL INTERVALS

Final averages, first and second series (cf. Table II); all observers

| Interval | Av. |
|---|---|
| Quartertone | 3.550 |
| Min. 2d | 2.700 |
| + Min. 2d | 2.375 |
| Maj. 2d | 2.325 |
| + Maj. 2d | 2.800 |
| Min. 3d | 3.029 |
| + Min. 3d | 3.154 |
| Maj. 3d | 3.338 |
| + Maj. 3d | 3.092 |
| Fourth | 3.212 |
| +Fourth | 3.233 |
| Tritone | 3.325 |
| + Tritone | 2.900 |
| Fifth | 3.212 |
| + Fifth | 2.946 |
| Min. 6th | 3.504 |
| + Min. 6th | 3.437 |
| Maj. 6th | 3.321 |
| + Maj. 6th | 2.650 |
| Min. 7th | 3.000 |
| + Min. 7th | 2.500 |
| Maj. 7th | 2.375 |
| + Maj. 7th | 3.525 |
| Octave | 4.600 |

| | |
|---|---|
| Average of non-musical intervals, 1st. Series | 3.015 |
| Average of musical intervals,* 1st. Series | 3.044 |
| Average of non-musical intervals, 2nd. Series | 3.130 |
| Average of musical intervals, 2nd. Series | 3.226 |
| Average of non-musical intervals, both Series | 3.057 |
| Average of musical intervals,* both Series | 3.110 |

*Excluding the octave.

*Reports.* First of all, a series of 12 musical intervals and about three times as many intermediate intervals, chosen at random, were presented with instructions for the *O* to judge each interval with regard to fusion on a positive scale of seven points. The results were not tabulated. As three of the *O*s made use of but four or five points of the scale, it was decided that a scale of five points would be sufficient, and the *O*s were given written instructions accordingly.

Here they were asked, as in the preliminary trials of the previous year, to judge upon unitariness or fusion. This was done in order to allow each

*O* to form a good criterion of judgment either from the meaning 'unitary' or from the meaning 'fused;' but again we found that 'fusion' was an unsuitable term: it was too general a term to W; it was determined by the active analyzing attitude for MP, and by the passive attitude for E and P. 'Unitariness' in general meant the passive attitude for all the *O*s wherever analysis and attentiveness were distinguished. The series was completed with the instructions expurgated so as to include only the word 'unitariness.' The judgment *five* was to be given for complexes "entirely unitary" and the judgment *one* for those "not at all unitary, like two separate things." The second limited series was given with the same instructions. The stimuli and results for these two series appear in Tables II and III.

During the second series, when the *Aufgabe* of unitariness seemed to be completely set, the *O*s were given a few random stimuli with instructions to "characterize the individual nature of each of the numbers used as response words." It seems worth while here to quote the report of W for its bearing on the matter of roughness. He distinguished the numbers as follows:

5—A clear single tone.
4—One clear tone; another, weaker, just standing out; *or* quality suggested as in dumb-bell effect.
3—Two tones standing out from each other.
2—More complex, with three outstanding tones; *or* two tones standing out distinctly.
1—Several tones of varying intensity; *or* two tones completely separated.

It is seen that this *O* made a *double classification* for non-unitary dyads as they have and have not difference tones; the former appear as a bunch of tones of different intensity, the latter as two tones becoming less and less intimate. He said, "roughness is well abstracted from but tends to reduce unitariness half a step."

*Introspective Factors.* (1) *Simple unitariness.* This seems to distinguish the best tonal fusions from all the rest. It is represented as *occlusion* of one tone by the other. It is not a uniform process: for P the higher tone predominates; for E it is the lower. Probably more than one process is at work.

(2) *Attitudinal factors.* (a) There may be the inability to analyze although complexity is perceived; E so described the typical judgment '4.' (b) A certain modicum of attention is necessary to, and brings with it, analysis. This attention must be on the complex, and yet apparently not on the more intense component *per se.* (c) Double meaning seems to occur with slow beats. MP stated that the quarter-tone may be judged '1' if it means oscillation back and forth from one tone to another, while it may be judged '5' if it means the oscillation of a single tone. (d) Other double meanings which are more complex and less easily described are frequently noted. These effects can be due to fluctuations in the center of attention, as in (b), or to fluctuations in meaning, as in (c).

(3) *The coherence factor.* In introspection this factor seems to work both ways: at times coherence connotes fusion; again disparity leads to fusional effect.

(4) *Roughness.* This is the most prominent factor and the most easily dealt with. It seems to be due entirely to fast beats or difference tones, and either enters frankly into the judgment, or is carefully abstracted from.

The fact that W maintained a double classification, one for those judgments with difference tones which stand out, and the other for those in

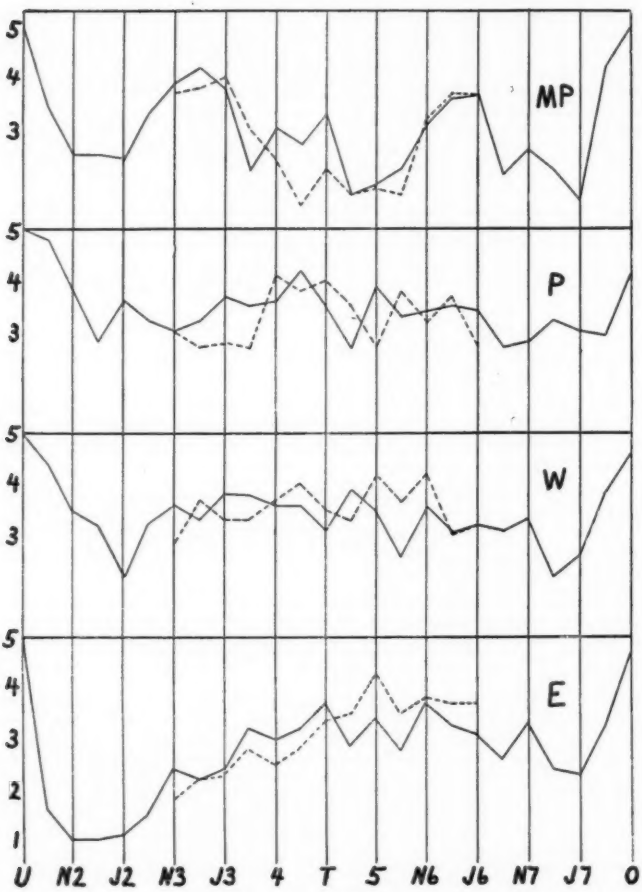


FIG. 2. DEGREE OF FUSION OF 24 INTERVALS AS JUDGED BY EACH OBSERVER

The musical intervals occur at the vertical lines; their intermediates are between the lines. The numbers on the ordinate scale indicate the average of the *O*'s judgments. The continuous line indicates the first series; the broken line is the second series. U = unison; N = minor; J = major; T = tritone; O = octave.

which both components are clear, tends to show that fusion depends upon much more than simple difference tones and the other superficial roughness factors which are at the root of Helmholtz's and Krüger's theories.

In the introspections, we find with surprising frequency the major third mentioned as being rough—more often in fact than most of the non-musical intervals.

(5) *Richness and brightness.* These factors are chiefly characteristic of the intervals in the middle of the octave.

(6) *Factors of association and familiarity.* Here we can only quote W's statement that " although fusion does not follow musical properties, they disturb judgment and adherence to the criterion."

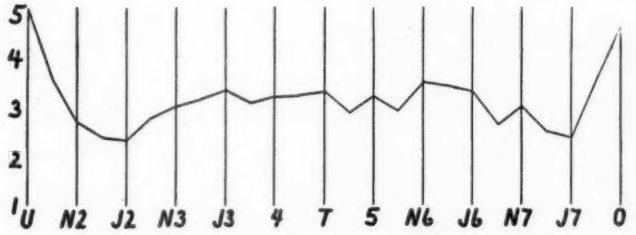


FIG. 3. AVERAGE OF CURVES IN FIG. 2

Average of judgments on each interval by all *O*s. The musical intervals occur at the vertical lines; their intermediates are between the lines. Cf. Fig. 2.

*Results.* A first scrutiny of the general results as represented in Figs. 2 and 3 shows clearly not only that some non-musical intervals are as well fused as certain musical intervals, but also that (when the stimuli are comparatively pure tones, at least) all the intervals make a more or less smooth curve, with the degree of fusion changing constantly and smoothly as the interval is slowly altered to make it wider or narrower. If we examine the final curve or any of the others for any sort of Stumpfian hierarchy of fusions, we find nothing of the kind. It is true that the octave heads the list, but we find the fifth subordinated to no less than four of the 'dissonant' intervals!

This makes it seem clear that simple vibration-ratio is not the important determinant of fusion that it has been supposed to be. Nor does the fact of familiarity appear to tell the entire story. It seems at the first glance as if we had indeed a number of independent factors, each determining a different set of fluctuations. The meaning of the Stumpfian results will be discussed later, together with a further consideration of our first evident conclusions.

If we examine the curves for the individual *O*s, recalling that one of the superimposed curves represents the pure musical intervals and the other the tempered scale, we find no significant difference in the two sets of results. With two of the *O*s the tempered fourth is better fused than the pure fourth; with the other two the reverse is the case. Practically the same may be said of the other musical intervals.

We can study the question of increased fusion of a complex as it becomes more familiar. Taking averages of the judgments on 6 non-musical in-

tervals in the first series (when they were absolutely unique) and in the second series, we get the following results:

| | MP | P | E | W |
|---|---|---|---|---|
| First Series | 2.8 | 3.4 | 2.9 | 3.4 |
| Second Series | 2.6 | 3.4 | 3.1 | 3.5 |

This lack of increase along the scale is not due to a downward movement of the whole scale, for it rose some two-tenths of a point.

Since some degree of habituation might have taken place earlier, the results of the most musical *O* were studied to determine whether there had been any growth of fusion of the unfamiliar intervals within the first series. Comparison of the first five judgments with the last five, showed that on the average the musical intervals became more unitary and the unfamiliar ones less unitary.

In general, the depressions in the curves correspond to those points on the bitonal scale where roughness occurs. This relation holds for the region of the seconds and sevenths, and explains the depressions on either side of the perfect fifth which do not, however, appear with every *O*. If we examine the tonometer curve in Fig. 1, we shall observe that the depressions are much more sudden. The isolated peak between the sixths on this curve is between the roughness area around the octave and that around the fifth.

It will be seen that the curve for W, who abstracted from roughness, shows these depressions in lesser degree than any of the others.

The peak at the minor seventh is hard to explain. It is noticeable in the results of all but P. A few introspective reports mention unitariness in the sense of 'occlusion,' but no further hint is given.

The general trend of the curve of Fig. 3 is up as the interval becomes wider, but if we examine the curve of Fig. 1 for the tonometer, we notice that there the general trend is down. Now if we look at the separate curves in the later series, we see that the general upward trend is due almost wholly to the influence of E, who built himself a trough of complete disunity around the seconds and in general tended to rate intervals higher as they were further apart. The opposite tendency, displayed in Fig. 1, is shown clearly by other *O*s. Thus the factor of coherence appears to be very questionable, for until we can describe two such processes we shall have to watch one process work at different times in opposite directions.

The experiment ended with a short series of relative judgments, which were taken to bring out any flagrant bivalency involved in the attitude in absolute judgment, whereby the scale used in judging intervals of simple ratio might differ from that used in judging the more complex intervals. A very limited part of the scale was used. Four series were taken, using eight intervals in complete series of paired comparisons.

MP and W both stated that in relative judgments such factors as roughness assumed more importance. In giving the pairs of intervals, one was always pitched much higher than the other. All the *O*s at once formed a prejudice in favor of either the higher or the lower complex.

It is hardly necessary to refer to the results more than to remark that the tritone ranks high, that the fifth occupies a singularly subordinate position, and that the non-musical intervals do not show any particular lack of unitariness.

*Conclusions.* The data thus briefly surveyed enable us to draw certain general conclusions with regard to the problem of tonal fusion.

(1) At least a great deal of what goes to make up fusion is not dependent on simplicity of vibration ratio. Krüger's difference tones enter into the perception in a striking degree, but all *O*s seem to be able to abstract from them and still to find many degrees of fusion. Roughness as a general phenomenon touches general parts of the field.

Immanent factors, too, seem to be ruled out. 'Micropsychic' rhythms no longer have aught to explain. Watt's nice adjustment of balance becomes rough and but massively symmetrical action of the *Gestalt* sort. The factor of similarity works well enough in the case of the octave, but, as Revész would expect, plays no part elsewhere.

With regard to Stumpf's results, we must note that in his work stimuli full of overtones were presented rapidly and judgments were given of 'one' or 'two' main tones. This sort of thing amounts to recognition of a given set of partials, and naturally the more like a more intense overtone the upper tone is, the more likely it is to be judged part of the lower tone ('one'-judgment).

TABLE IV

RELATIVE JUDGMENTS FOR MUSICAL AND NON-MUSICAL INTERVALS

Table shows number of judgments favoring each interval

| Interval | Observers MP | P | E | W | Total |
|---|---|---|---|---|---|
| Fourth | 13.5 | 14.0 | 14.0 | 11.0 | 52.5 |
| + Fourth | 10.0 | 14.5 | 10.5 | 15.0 | 50.0 |
| Tritone | 11.0 | 17.0 | 13.5 | 17.0 | 58.5 |
| + Tritone | 13.5 | 11.0 | 9.5 | 12.5 | 46.5 |
| Fifth | 11.0 | 13.5 | 15.5 | 13.0 | 53.0 |
| + Fifth | 12.5 | 16.5 | 14.0 | 16.0 | 59.0 |
| Minor Sixth | 21.5 | 12.0 | 17.0 | 15.0 | 65.5 |
| + Minor Sixth | 19.0 | 19.0 | 18.0 | 12.5 | 63.0 |

| | |
|---|---|
| The musical interval judged more unitary: | 229 times |
| The non-musical interval judged more unitary: | 219 times |

(2) As to the question of growth in degree of fusion through familiarity, it appears, in the first place, that the degree of fusion of wholly unfamiliar intervals was initially quite high; and, in the second place, that no increase of fusion could be noted during the course of the experiment.

(3) Thus we are reduced to a consideration of fusion of the simplest sort as a multiplication of factors. We may presume that pleasantness is not one of these factors, and that to bring in some ultimate fact of fusion —a Kempian peculiarity of the sensory material—would be no more than a confession of ignorance. We no longer need any vague factors to explain the hanging together of certain scattered intervals throughout the bitonal range.

First among these factors stands roughness. This factor is plainly a determinant of fusion, but a very instable determinant.

Richness and brightness and like factors were not brought out any too well by the procedure in this experiment, but it may be presumed that certain preferences, like the interval just above the fifth to the fifth itself, may be due to this system of meanings; and individual introspections on this particular case seem to bear this statement out.

Coherence-disparity as a criterion, seems to have a double aspect. Proximity of pitch is well known to have an associative effect in melody.[41] It has a definite fusional effect with some *Os*, and is so noted in introspection.

Familiarity supposedly plays some small part in 'fusion.' Musical properties disturb the judgment. It may be true that *nothing* was familiar on the Stern variators. Against this possibility and also against the con-

[41] O. Ortmann, *Psychol Monog.*, 35, 1926, no. 162.

tention that non-musical intervals might subjectively, before judgment, be assimilated to adjacent musical intervals, we can oppose the wholly unsolicited reports of two *O*s that "about half of the intervals given seemed to be not musical."

The significant phenomena in our results appear as slight and constant fluctuations rather than as sharp and definite changes, and thus take us back for comparison to Wertheimer's factors of configuration in the visual field. In the tonal complexes the intimacy of grouping varies, and it varies with the interplay of a number of definable factors. Intervals would thus seem to be configurations of exactly the same sort as groupings in the visual field.

## IV. Summary

(1) If we recognize the fact that there are *degrees* of fusion, we find that the most non-musical intervals are as well fused as the most musical intervals, including the perfect fifth.

(2) The simple intervals and those on the tempered scale are equally well fused.

(3) Fusion is not all dependent upon simplicity of vibration-ratio.

(4) Pleasantness is not a determinant of fusion, although fusion may, of course, determine pleasantness.

(5) Familiarity has little bearing on fusion. Similarity of the two components leads to some effect, but this takes place only in the case of the octave.

(6) A fusion is a configuration, and as such is the resultant of the interplay of various aspects of the interval.

# MUTUAL INTERFERENCE IN THE EVOLUTION OF CONCEPTS

By J. A. GENGERELLI, University of Wisconsin

Long ago, James, with his usual clearness, described an act of conception as resulting "from our attention singling out some part of the mass of matter for thought which the world presents, and holding fast to it without confusion."[1] Stated in behavioristic terms, we would probably omit the factor of 'attention' as a causal agent, and say simply that in conception the organism, as a result of a process of conditioning, responds to a stimulus pattern which constitutes a part of the total object as it exists in the external world. Of James' description of the process of evolution by which we obtain the concepts we use in everyday life, everyone knows. "What is associated now with one thing and now with another tends to become dissociated from either," he writes, "and to grow into an object of abstract contemplation by the mind."[2] "One might," he continues, "call this the *law of dissociation by varying concomitants.*"

Regardless of how the above description has been accepted by modern psychologists, no one, to our knowledge, had undertaken an adequate experimental investigation of the evolution of concepts until recently. Hull, however, using a modification of Chinese symbols, carried on a rigorous piece of research which corroborates in the essentials the evolutionary process as described by James;[3] but such an investigation of the problem as Hull's at once suggests the problem most closely associated with it—that of the mutual *interference* of concepts in the process of their evolution.

Here again, it is James who most clearly and convincingly states the facts. The following passage is classic. "Every reality has an infinity of aspects or properties. Even so simple a fact as a line which you trace in the air may be considered in respect to its form, its length, its direction, and its location. When we reach more complex facts, the number of ways in which we may regard them is literally endless. Vermillion is not only a mercury-compound, it is vividly red, heavy, and expensive, it comes from China, and so on, *ad infinitum.*"[4]

Thus at any given moment we react only to certain given parts or aspects of an object and ignore the rest. In fact, with respect to some of the things with which we come in contact daily, we have become so accustomed to conceiving of them or reacting to them in such definite stereotyped ways that it is only with difficulty that we can be brought to have a different 'slant' on them. As our author quaintly remarks, "we are so stuck in our prejudices, so petrified intellectually, that to our vulgarest names, with their suggestions, we ascribe an eternal and exclusive worth."[5]

It is at this point that the problem arises. Is it really true that if we have learned to conceive a thing in a certain way, it will be relatively more difficult to conceive that same thing in a second and different way? More specifically, would more effort be required to conceive the thing in the second way, after having first learned to react to it the first way, than if the first way had not been learned at all.

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[1] W. James, *Principles of Psychology*, I, 461.

[2] *Ibid.*, p. 506.

[3] C. L. Hull, Quantitative aspects of the evolution of concepts, in *Psychol. Monog.*, 28, 1920, 1-85.

[4] James, *op. cit.*, II, 332.

[5] *Ibid.*, 334.

## METHOD

Would it, therefore, be more difficult for a group of *Ss* to evolve the 9 'concepts' or common elements contained in the material shown in Fig. 1, taken horizontally, after having evolved nine other 'concepts' or common elements from the *same* material, taken vertically?[6] To take account of the practice effect involved, a control group of *Ss* was made to evolve, successively, the 'concepts' contained in the material shown in Figs. 3 and 4.[7] By subtracting the average amount of labor required by the control group to evolve 'concepts' or common elements contained in the material in Fig. 4 from the average amount required to evolve those contained in Fig. 3, we get a quantitative expression of the practice-effect alone. If we likewise subtract the average amount of effort required by the test group to evolve the set of 'concepts' contained in the material in Fig. 1, taken horizontally, from the average amount required to evolve the set of 'concepts' from the material, taken vertically, and subtract this difference from the difference obtained in the case of the control group,[8] we get a result expressive of the amount of interference involved, if the factor of interference is really present.

The cardboard squares on which the symbols were drawn, as shown in Figures 1, 3, and 4 were cut into 81 2-centimeter squares, which were then posted on a long narrow band of oilcloth, about 2½ centimeters apart. Care was taken, however, that no two symbols having the same common elements should follow each other consecutively. The exposure apparatus is that described in Hull's work.[9]

*S* was seated comfortably before the apparatus, the aperture at a level with his eyes. He was then handed the following typewritten directions. "I have here in this apparatus a number of figures of different designs. I will expose every figure for 5 sec., and shortly after it is exposed I will pronounce a letter of the alphabet, which you will pronounce aloud after me. The purpose of this experiment is to see how long it takes you to associate certain letters of the alphabet with certain figures which will be shown you. If, after you've observed a figure a few times you think you know the letter, which is associated with it, then speak the letter aloud before I do, so that I can tell whether you know it or not. If you are not quite sure whether a certain letter is associated with a certain figure, then guess. This experiment is not very hard, and, besides, you will be given a short rest every

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[6] It will be observed that every symbol has two possible common elements. As will be seen from Fig. 2, each significant element or character is to be associated with a certain letter of the alphabet. Taken vertically, therefore, all the symbols contained in the first left hand column of Fig. 1 were to be associated with the letter 'C;' those in the second column with the letter 'I'; those in the third column, with 'R' and so on to those in the ninth column, which were to be associated with 'E.' Taken horizontally, all the letters in the top row were to be associated with 'K;' those in the next row, with 'Y;' those in the third row with 'N,' and so on. The *Ss*, both test and control evolved, consequently, two series of 'concepts' successively; these we shall designate as the *initial* and *secondary* series, respectively.

[7] The possibly unequal difficulty of the two series of common elements to be evolved was met, by having one-half of the test *Ss* evolve series A—the series drawn horizontally across the square in Fig. 1— as the initial series; and series B— the series drawn vertically,—as the secondary series; and the other half to proceed the other way about. The same, of course, was observed for the control group—one-half of the *Ss* using $A_1$ as the initial series; the other half $B_1$.

In order to make the material used by the control group as much like that used by the test group as possible, 81 small figures, each different from every other, were drawn to take the place of one of the series of common elements. By consulting Fig. 1, it will be observed that the symbol in the upper right-hand corner, for example, consists essentially of a parallelogram with a 'three-leaf' in the center and an 'X' on the outside. The same symbol in the corresponding position in Fig. 3 shows the 'three-leaf' on the inside in the same position, but on the outside, instead of the 'X,' a new figure. In Fig. 4, it is the 'three-leaf' which is absent, its place being taken by the figure, and the 'X' is on the outside as in the test material.

[8] We assume, of course, that the two groups of *Ss* are comparable in ability.

[9] *Op. cit.*

few minutes.[10] During these intervals you are urged to keep your mind off of the material."

Every *S*'s record showed the number of 'prompts' required to evolve a given series of 'concepts.' As every symbol appeared in the aperture of the exposure apparatus, *E* pronounced the letter to be associated with the significant element contained in that symbol, and this counted as a prompt. Whenever *S* responded correctly to the symbol exposed, *E* would say 'right,' and, of course, no prompt was given. If an incorrect letter was given, *E* supplied the correct one, and charged the *S* with a prompt.

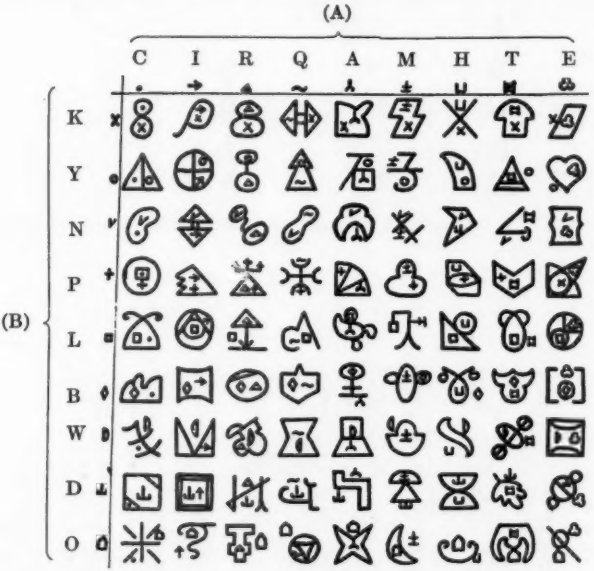


FIG. 1

The criterion adopted as indicative of complete learning, or rather, the successful evolution of the given series of 'concepts,' was that the *S* go straight through the list and respond correctly to every one of the 81 symbols.[11]

[10]After the 27th and 54th symbol, the exposure apparatus was stopped for a period between 15 and 20 sec., and the *S* asked to rest his eyes by letting them wander about the room. At the end of the series of 81 symbols, that is, before the same series was repeated, *S* was given a rest between 45 and 60 sec.

[11]The question may be asked at this point as to how assurance was obtained that the *S*s responded to the 'dynamic core,' so to speak, in the symbol-complex rather than to the symbol-complex as a whole. It was ascertained that, as a matter of fact, the *S*s found it impossible to comply with the criterion by 'brute memory' in the time allowed. There were several *S*s who tried this and succeeded to a meager extent, but they soon gave it up. Whenever an *S* was responding to the total symbol-complex, his responses were always hesitant and his errors very erratic. Furthermore, when the experiment had been finished, the *S*s invariably made some such remark as "I had an awful easy time singling out that arrow at first," or "It's easy after you get on to the idea;" which leaves no doubt as to the nature of the process.

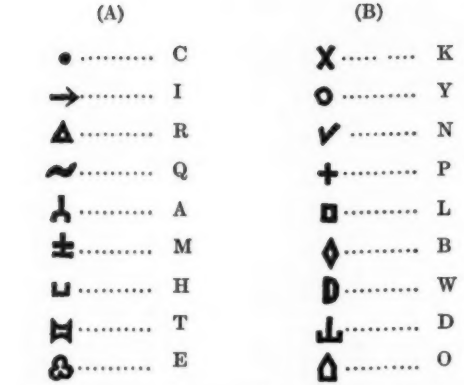


FIG. 2. The two series of 'concepts' (common elements) and the letters to be associated with each.

FIG. 3

RESULTS

The results of the experiment are given in Table I. It will be observed that 24 *Ss* constituted the test group and 24, the control group. The table gives the scores of every *S* individually for both the initial and secondary series of 'concepts.'

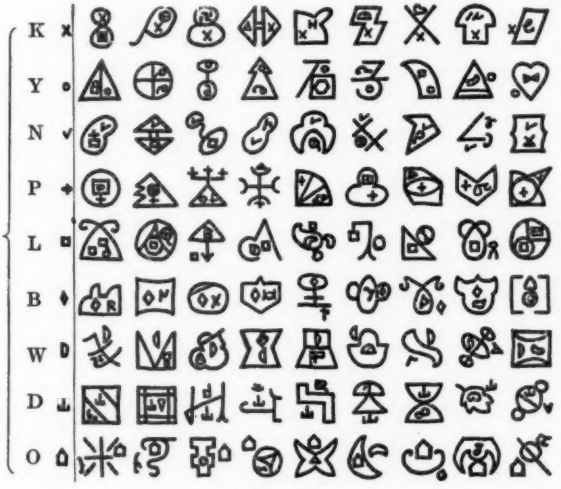


FIG. 4

It is obvious, that the average number of prompts required by the test group to evolve the secondary series of 'concepts' must necessarily be much smaller than the average number required for the initial series. For, after having evolved the initial series, the *Ss* would no longer stare at each symbol exposed in its entirety, but would seek out the significant element. In the words of one of the *Ss*, they would know that "it isn't the whole thing that counts, but just a part of it." This is true, of course, for the control group as well. The control *Ss* evolved the same 'concepts' from the same material, but the material, in their case, was so prepared as not to give rise to any interference; so that if our set-up was adequate and the factor of interference really present in the case of the test *Ss*, we should expect that the average number of prompts required by that group to evolve the secondary series of 'concepts' would be distinctly larger than the mean number required by the control *Ss* to evolve the secondary series. Table I shows this to be the case. The average number of prompts for the test group being 86, and for the control group, 57. There is a difference of 29 prompts, with a probable error of 7.9. The difference is 3.67 times greater than its probable error.

A second and better way of computing the results, is to compute for every *S* the percent reduction in the number of prompts required to evolve the secondary series over the number required for the initial series. We may thus obtain the mean percent reduction for each group of *Ss*. Here, if the factor of interference really exists in the process under consideration, we should expect the mean percentage decrease for the control group to be

distinctly larger than the mean percent decrease for the test group. Reference to Table I shows, in fact, the mean percent decrease for the control group to be 75, that for the test group 57. A difference of 18 points, with a probable error of 3.86, the difference being 4.66 times greater than its probable error.

TABLE I

| | Test Group Concepts from material in Fig. 1 | | | | Control Group Concepts from material in Figs. 3 and 4 | | | |
|---|---|---|---|---|---|---|---|---|
| | No. prompts required Initial Series. | Secondary Series | % Decrease* in Secondary | | No. prompts required Initial Series. | Secondary Series | % decrease* in Secondary |
| Evolving series A as initial series; B as secondary | 61 | 38 | 38 | Evolving $A_1$ (Fig. 3) as initial series; $B_1$ (Fig. 4) as secondary | 152 | 55 | 64 |
| | 153 | 140 | 8 | | 277 | 113 | 59 |
| | 291 | 167 | 43 | | 124 | 51 | 59 |
| | 79 | 29 | 63 | | 138 | 79 | 43 |
| | 118 | 61 | 48 | | 442 | 88 | 80 |
| | 138 | 70 | 49 | | 317 | 44 | 86 |
| | 254 | 100 | 61 | | 143 | 81 | 43 |
| | 374 | 125 | 67 | | 117 | 37 | 68 |
| | 271 | 80 | 70 | | 453 | 130 | 71 |
| | 177 | 122 | 31 | | 206 | 40 | 81 |
| | 197 | 81 | 59 | | 370 | 110 | 70 |
| | 219 | 219 | 0 | | 62 | 21 | 66 |
| Evolving series B as initial series; A as secondary | 159 | 46 | 71 | Evolving $B_1$ (Fig. 4) as initial series; $A_1$ (Fig. 3) as secondary | 243 | 56 | 77 |
| | 263 | 88 | 66 | | 89 | 21 | 76 |
| | 160 | 41 | 74 | | 386 | 37 | 90 |
| | 348 | 74 | 79 | | 417 | 40 | 90 |
| | 343 | 77 | 77 | | 324 | 31 | 90 |
| | 263 | 84 | 68 | | 345 | 73 | 79 |
| | 171 | 38 | 75 | | 459 | 60 | 87 |
| | 208 | 36 | 82 | | 450 | 31 | 93 |
| | 515 | 27 | 95 | | 301 | 68 | 77 |
| | 96 | 69 | 28 | | 95 | 31 | 67 |
| | 298 | 195 | 35 | | 280 | 27 | 90 |
| | 384 | 55 | 85 | | 235 | 35 | 85 |
| Ave. | 231 | 86 | 57 | Ave. | 268 | 57 | 75 |
| m.v. | 88.75 | 39 | 19.5 | m.v. | 112 | 24.4 | 11 |
| P.E.n | 15.3 | 6.7 | 3.36 | P.E.n | 19.3 | 4.2 | 1.9 |

*In computing the decrease, anything over .5 was counted as 1.

To the question, then, as to whether it is more difficult to conceive a thing in a second way after having first learned to conceive it in another way, than if the first way had not been learned at all, the results of this experiment would lead us to answer in the affirmative.

Although the experimenter made no systematic effort at securing introspections from the *Ss* as to the nature of the interference or conflict they experienced, a number of *Ss* were questioned on this point after the secondary series of 'concepts' had been evolved. All admitted being confused and at loss for at least the first 18 or 20 syllables. They expressed themselves as still responding to the original common elements during this time, and not knowing what to make of the fact that the common element which in the previous series had been designated by one letter was now apparently designated by another.

It will also be noticed from Table I that the initial series for the control group has a slightly larger mean than the initial series of the test group. There is a difference of 37, but the probable error, 24.6, is so high, that the difference is hardly due to any difference in the difficulty of the two sets of material used. But even if the two sets of material used by the two groups were of unequal difficulty, if we assume that the two groups were comparable in ability—and we feel that there is every reason for this assumption,

TABLE II

| The number of prompts required | | The number of prompts required | |
|---|---|---|---|
| Evolving as initial series the common elements (A) from the material in Fig. 1 | 61 | Evolving as initial series the common elements (B) from the material in Fig. 1 | 159 |
| | 153 | | 263 |
| | 291 | | 162 |
| | 79 | | 348 |
| | 118 | | 343 |
| | 138 | | 263 |
| | 254 | | 171 |
| | 374 | | 208 |
| | 271 | | 515 |
| | 177 | | 96 |
| | 197 | | 298 |
| | 219 | | 384 |
| Evolving as initial series the common elements ($A_1$) from the material in Fig. 3 | 152 | Evolving as initial series the common elements ($B_1$) from the material in Fig. 4 | 243 |
| | 277 | | 89 |
| | 124 | | 386 |
| | 138 | | 417 |
| | 442 | | 324 |
| | 317 | | 345 |
| | 143 | | 459 |
| | 117 | | 301 |
| | 453 | | 95 |
| | 206 | | 280 |
| | 370 | | 235 |
| | 62 | | 450 |
| Ave. | 214 | Ave. | 285 |
| m.v. | 94.3 | m.v. | 92 |
| P.E.$_n$ | 16.2 | P.E.$_n$ | 15.8 |

285 − 214 = 71 difference
P.E.$_D$ = 22.6

as the *Ss* were selected from the introductory course in psychology and divided into two groups haphazardly—the validity of the results expressed in terms of the average percentage decrease would not be disturbed, since these express relative and not absolute values.

Table II contains results that justify the technique used with the intent purpose of equalizing difficulty of the two series of common characters or 'concepts' used. The first column gives the results for those *Ss*, regardless of the group to which they belonged, who evolved as the initial series the common characters (A) or ($A_1$) from the material in Figs. 1 and 3 respectively; the second column, for those evolving as the initial series, the common characters (B) or ($B_1$) from the material in Figs. 1 and 4, respectively. As will be seen, there is a decided difference in the degree of

difficulty of the two series of common characters. A difference of 71, with a probable error of 22.6—the difference is 3.14 times greater than its probable error. This is exactly what could have been predicted from the behavior of the *S*s during the experiment. Nearly all the *S*s had greater difficulty with the series of common elements drawn vertically in Fig. 1. They invariably confused at first the character 'X' (K) with the character '+' (P), and the character '◇' (B) with the character '□' (L).

A question that suggested itself again and again during the course of the experiment was whether the individual who obtained a high score in the evolution series—*i.e.*, who required a small number of prompts—also secured a high score in the conflict series. The coefficient of correlation was therefore calculated between the initial and secondary series for the test as well as the control group.

The r between the initial and conflict series for the test group of *S*s was $+ .151 \pm .134$; for the control group, $+ .3644 \pm .119$. The difference between the two 'rs' is .2134, and the probable error of the difference is $\pm .178$. There seems, then, to be a very low correlation between the initial and conflict series for the control group of *S*s, whereas, for the test group no relation seems to exist; that is, there is no possibility of predicting what any given individual will do in the conflict series by what he has done in the evolutionary series.

It might be of interest and significance to find out whether over-learning would influence the amount of interference occurring in the secondary series of concepts one way or the other. For in actual life, with respect to our everyday concepts, because of the vast amount of repetition which they undergo, "we are so stuck in our prejudices, so petrified intellectually" that we can't observe anything but that one side of the complex object which we have been taught to recognize. Now, if in having the *S*s evolve the initial series of 'concepts,' we were to use as the criterion of the satisfactory evolution of the series, not that the *S*s go straight through the list of 81 symbols *once* without a single prompt or error, but that they go, say, ten, twenty or thirty times without a single prompt or error, would the amount of interference be greater or smaller? In such an experiment a greater number of *S*s would very probably be needed to insure a low probable error.

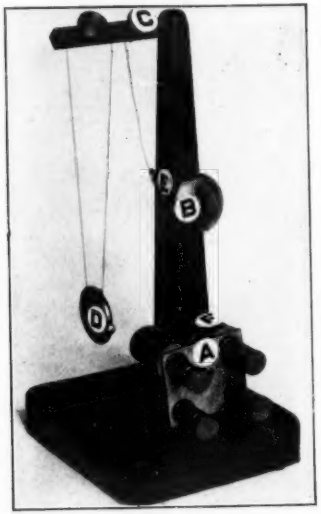


FIG. 1

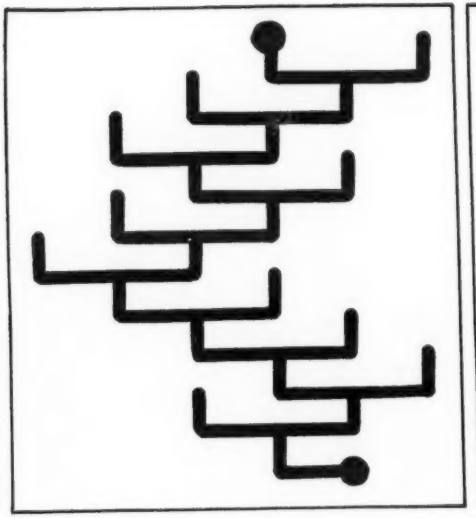

FIG. 2

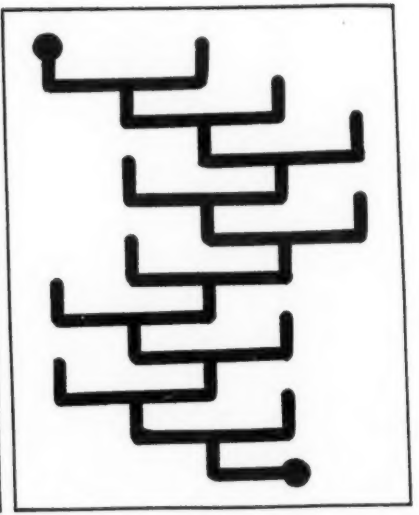

FIG. 3

# APPARATUS

## SOME SUGGESTIONS IN LABORATORY APPARATUS

By CHRISTIAN A. RUCKMICK, University of Iowa

### I. SANFORD CHRONOSCOPE WITH ELECTRICAL RELEASES

The writer carried on some correspondence with the late Dr. E. C. Sanford in regard to an electrical release for his well-known reaction chronoscope. Sanford was very enthusiastic about the proposal and stated that when he first devised the apparatus he had an electrical release in place of the usual mechanical key. The reason why it was discontinued, he said, lay in the fact that at that time the mechanical key was more suitable for the psychological laboratories of that day. Since then, with the improvement of laboratory technique, there has been more need for an electrical release, especially when the observer is removed from the room in which the control apparatus is situated. But even under the ordinary conditions when *E* and *O* work in the same room, the release can be made more quiet and quick through electrical keys than in the mechanical type. Accordingly, at the psychological laboratory of the University of Iowa, we are using only chronoscopes with electrical releases.

These chronoscopes were made by the departmental machinist in accordance with suggestions from the writer and include a number of improvements. The illustration, Fig. 1, shows most of the essential features. At A are located the two electro-magnets with binding posts mounted on insulated material. One of the magnets is marked F. Just below A is an adjusting screw which regulates the relative position of the magnets since the whole square may be slightly rotated in order to bring the magnets into the optimal position with relation to the bobs. These bobs (B and D) are made of brass with iron rivet-head inserts opposite the cores of the magnets. The length of the threads attached to the bobs is regulated by two thumb screws one of which is illustrated at C. The silk threads pass through holes in the horizontal brass bar suitably beveled to prevent cutting the threads and are wound around the ends of the thumb screws in opposite directions so that a single turn of the screw winds up both strings equally for each bob.[1] When the instrument is carried from place to place the bobs may be hung up on hooks indicated at E on the vertical pillar. The construction throughout is rugged and is finished with the new crystalline type of japanning. Three small rubber feet protect the tops of tables on which the instrument may be used. Obviously, the release operates on a break circuit, but through proper instructions to the observer the current can be economically used. One dry cell in good condition is ordinarily sufficient to operate the magnets. Of course, specially treated iron cores must be used to prevent permanent magnetization.

### II. MODIFIED FORMS OF THE STYLUS MAZE

Some years ago Warden[2] suggested a form of stylus-maze which he used in connection with a special study in learning. The form which he used is illustrated in Fig. 2. It was suggested to us that this type of apparatus might be suitable for laboratory exercises in the field of motor

[1]This instrument was perfected several years ago. Since then a note on a somewhat similar improvement in the adjusting screw has been published by S. W. Fernberger, (this JOURNAL, 37, 1926, 154-155). The attachment of the threads to the thumb-screw is almost identical with Fernberger's arrangement. In both cases the idea is to adjust the length of the bob without giving it a cant.

[2]C. J. Warden, The relative economy of various modes of attack in the mastery of a stylus maze, *J. Exper. Psychol.*, 7, 1924, 243-275.

learning. In the Iowa laboratory, since many students in the college of education enroll for the purpose of acquiring a more intimate knowledge of psychological laboratory technique, we have frequent occasion to stress experiments in learning. For this purpose we have made two types of maze—one illustrated in Fig. 2 following closely the dimensions specified in Warden's article. The maze is made of $\frac{1}{8}$ in. sheet brass and measures $12\frac{3}{4}$ x $11\frac{3}{4}$ in. The width of the path is $\frac{1}{4}$ in. throughout. The circles at the beginning and end are $\frac{3}{4}$ in. in diam. This type of maze has the advantage of presenting practically two discreet problems when both sides are used. To vary the procedure we have devised another similar maze illustrated in Fig. 3.

The maze may be used for a variety of procedures. Two, which we commonly use in the laboratory, are to time the number of errors and the time of running the maze with a stylus or pencil without preliminary visualization as compared with learning to run the maze with a 5-sec. previous visual exposure. It is not necessary to use glass as a writing surface as is suggested in the article cited. A smooth surfaced table with a sheet of paper under the maze will give accurate results for our purposes. Instructions are preferably typewritten in either case.

We have also used the apparatus in connection with experiments on emotion when the effect of reward or punishment was used in connection with the number of wrong tracings—that is, into the blind alleys. Of course his could be a separate experiment in learning but our purpose was to create some fear of punishment in the form of electrical shock or scolding in order to get a description of the emotional state. Variations may include trying the test from the right and from the left in passing from entrance to exit as well as from bottom to top, and often top to bottom. In these cases the different types of learning will be brought out in so far as motor images conflict with visualization or in so far as one of the other types of imagery is used predominantly in the learning series. In all cases it is a good device for getting qualitative reports on the learning consciousness as well as the quantitative data.

## AN IMPROVED HEAT GRILL

By H. G. Bishop, Wittenberg College

All the earlier forms[1] of the heat grill have certain limitations to escape which the form described here and shown in Fig. 1 was devised. The dimensions, material, number of coils, etc. are not specified because such matters must be left to the needs of the specific experiments.

An inspection of the accompanying drawing, Fig. 1, shows three main units: the coils, the supporting stand, and the capstan. The coils are supported by silk fishing line running from the capstan spool through holes in the stand top to the coils themselves. The capstan is held up by a coiled spring which is sufficiently strong to support the load of the temperature coils. A pin through the shaft of the capstan limits the upward excursion.

The capstan is capable of two kinds of movement: it may be depressed and raised; or it may be rotated in either direction. If it is depressed from the neutral position shown in Fig. 1, both systems of coils will be lowered

[1] F. Cutolo, Jr., A preliminary study of the psychology of heat, this Journal, 29, 1918, 442-448.
E. B. Twitmyer and S. W. Fernberger, A demonstrational heat grille, this Journal, 38, 1927, 119.
N. C. Burnett and K. M. Dallenbach, The experience of heat, this Journal, 38, 1927, 418-431.

without altering their alignment in the horizontal plane, and both systems will simultaneously come to rest upon the arm of the observer as it lies beneath the supporting stand. If the capstan is rotated the position of the two temperature systems is altered reciprocally in respect to one another. Counterclockwise rotation, which is indicated, lifts one system—the clear—and lowers the other—the shaded. A slight turn is sufficient. When the capstan has been rotated through the proper angle and in the right direction, the system of coils desired will be handing freely below the other. If the capstan is then pushed downward the lower system alone will touch the observer. Clockwise rotation will, obviously, bring the

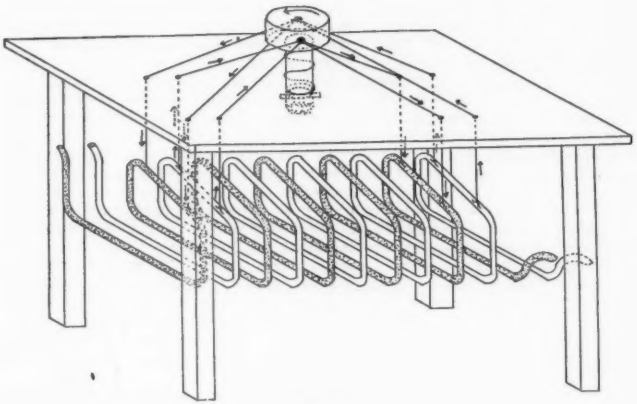

FIG. 1

systems back to their original positions ready for simultaneous stimulation by both coils or, if the rotation has been beyond the original setting of the capstan spool, by the hitherto unused system.

The following are advantages of this instrument over the earlier forms. (1) Stimulation may be rapidly altered between the warm alone, the cold alone, or by both temperatures simultaneously. (2) The temperature changes may be effected without the observer changing the position of his arm, which is not only advantageous in itself but which also permits repeated stimulation of the same part of the skin. (3) Since the coils, if the capstan is turned until the supporting strings are slack, will bear down with the amount of their own weight the stimulation will always be at the same pressure. Variations of the pressure of the arm upon the grill, such as Dallenbach noted,[2] are obviated by this arrangement.

[2] Burnett and Dallenbach, *op. cit.*, 425.

# SOME SUGGESTIONS REGARDING THE CONSTRUCTION OF APPARATUS AND TESTS

By W. C. WIDEMAN, C. H. Stoelting Co.

The economical manufacture of apparatus and laboratory supplies is of interest to all experimental psychologists. I therefore wish to bring to the attention of all those who devise psychological apparatus and tests, some of the unnecessary expenses that are frequently met by the manufacturer.

One of the most frequent sources of trouble and expense are the toys, pictures, and household specialties so often used in mental tests. A psychologist goes into some general or department store and selects some materials that seem to meet his requirements and then proceeds to devise his series of tests. In the natural course of events, if he has produced something of real value, it is not long before other psychologists wish to use similar material. About that time every one interested discovers to his astonishment and dismay that many of the articles are off the market. The particular novelty or specialty has had its day and is superseded by something else. Often the item that takes its place is quite different, and as more often happens, the item has been a commercial failure and the plates or tools have been 'junked.' Under these circumstances the apparatus manufacturer may have to go to the expense of producing new tools, or plates, and, as happens in some cases, pay the original producer a heavy royalty for the use of copyrighted plates or patented mechanical devices. When a thing is taken from the market the original producer can rarely be induced to supply it at anything like a reasonable price in the comparatively small quantities used for psychological purposes.

The same facts apply to supplies for demonstrational and experimental purposes. Electrical and spring motors, etc. have their hour and day and sooner or later are superseded by something more practical from a commercial point of view, but often not so well adapted for use as a part of a special psychological apparatus. Under the conditions of a changing commercial supply it would be more economical in the end if greater care were exercised in the selection of the materials.

So as to keep the cost of production down to a minimum, it would be advisable in devising new tests and apparatus, to determine in advance how long the item one contemplates using will probably be on the market in its present form, or else to make some arrangement with the original manufacturer for a continuance of the supply. If neither of these courses is possible, it would in many instances be economy in the long run to produce at the start the exact type of picture, device, or other material required.

## BOOK REVIEWS

*Psychological Types of the Psychology of Individuation.* By C. G. JUNG. Translated by H. G. Baynes. New York, Harcourt, Brace & Co., 1923, pp. xxii, 645.

The author finds in his "pratical medical work with nervous patients" (p. 9) some typical varieties, of which two are specially clear: *introversion* and *extraversion*. Description is embarrassed in that no individual is a pure type, each tending to compensate for his own one-sidedness; there is a merely relative predominance of one mechanism (13). The two phases indeed alternate with a kind of natural rhythm (systole and diastole); but one of them gradually dominates the conscious attitude while the other recedes into the uncounscious (11). No type is completely known until both aspects (conscious and unconscious) have been surveyed. These types are due to native differences and appear at random in all strata of society (413). In general the extravert finds satisfaction in the object, his social and physical milieu, which alone has significance for him; whereas the introvert is concerned with his own self and psychic processes, the environing world being incidental and secondary. The former adapts by fitting himself to the world, the other by protecting himself from the world (414); the former is the genius of civilization and practical achievement, the latter of culture and abstract invention (352).

(*A*) *The extraverted type.* The extraverted type consciously adopts the prevalent attitudes and code of his fellows, and represses contrary egoistic tendencies; but these break through from time to time and confuse the picture. Hysteria is the typical neurosis, with its exaggerated rapport (416 ff.).

(1) The *extraverted thinking* type tries to bring the whole of life into harmony with what he conceives to be world-law, as revealed in objective data; he seeks to regulate his activity by intellectual formulae, which leave no room for affective (irrational) tendencies; these outlawed feelings take revenge by undermining his conscious convictions, and he in turn overcompensates the doubts by fanatical belief and sacrifices (434 ff.).

(2) The *extraverted feeling* type is held in spell by objective standards of some sort; a thing is felt to be beautiful because that is the 'correct' way to feel about it. This attitude, found mostly among women, conduces to harmony in social life. Such a woman marries a man who is 'suitable;' this demands no simulation of love on her part because she just naturally falls in love with a suitable man. Thinking remains infantile, negative, and in all cases the servant of feeling (488 ff.).

(3) The *extraverted sensation* type is the most realistic of all; he lives in the concrete experience of the moment, and is ever seeking new sensations, new thrills. He has no purpose but 'carpe diem,' is likely to become credulous and captious, and condemns the restraints of reflection (457 ff.).

(4) The *extraverted intuitive* type depends upon sensation only as a guide for intuitive penetration. He is not satisfied with the sensory realities of a situation, as is the preceding type, but looks ever to its future possibilities; when a thing's novelty is exhausted he throws it aside; he is the adventurer, promoter, who is cramped and stifled in static conditions of life (464 ff.).

(*B*) *The introverted type.* The introverted type is controlled by subjective factors; the object is of concern only because of what it constellates or invokes in himself. It is quite unfair, however, to call him autoerotic or egoistic; because in every psychic function the subject is just as indispensable as the object. The assumed superiority to the milieu is compensated by an unconscious fear of it, which thwarts his inner freedom; he is terrified because the world moves without his permission or control. The prevailing philosophy of the West being extraversive, he feels oppressed

and so works ever harder to protect himself against the pressure of society; his typical neurosis is psychasthenia (471 ff.).

(1) The *introverted thinking* type is concerned less with new facts than with new theories or insight; he looks upon facts merely as evidence for his theories. He is scrupulous and painstaking in his work, stubborn in pursuit of his own ideals; he cares little about influencing others, as in teaching, and gives little heed to their judgments. He thus appears inaccessible and is likely to be misunderstood. Kant belongs here and Darwin belongs to the corresponding extraversive group (484 ff.).

(2) The *introverted feeling* type is represented largely by women, who are silent and melancholic; they find it even harder to communicate than does the introversive thinker. They are consciously indifferent and placid, but may compensate with overweening vanity and cruel ambition. The extreme cases are neurasthenic (492 ff.).

(3) The *introverted sensation* type never stops with the pure sensory effect of stimulation, but suffuses the percept with subjective additions; two artists of this type would paint the same landscape very differently. He tries to see a thing *sub specie aeternitatis* instead of with its direct, momentary aspect; he is likely to misinterpret what he perceives and to move in a mythological world (500 ff.).

(4) The *introverted intuitive* type perceives the background-processes of consciousness much as, in ordinary sensation, we perceive external objects. His own unconscious images become objects of perception to him and may seem so detached that he is quite unaware of their source in his own being. Here belong the prophets of Israel, the mystics and dreamers and seers, who live in the exuberant profusion of their phantasies. The outer expression is likely to be feeble and unimpressive to his contemporaries (a voice crying in the wilderness), but his posthumous influence may be very great (508 ff.).

In the early part of his book, Jung surveys many fields of life and thought seeking evidence to support his theory of opposing types. Thus, the strife of Tertullian and Origen in the early church arose because the introverted Tertullian, after a *sacrificium intellectus* (*certum est quia impossible est*), finds reality in feeling; whereas the extraversive Origen, who escapes from self by a *sacrificium phalli*, finds truth by intellectual apprehension of God (20 ff.). The realists who maintain that universals exist *ante rem* are introverted, the nominalists who derive them *post rem* are extraverted (37 ff.). The extraverted Freud finds adjustment in life by using external objects to satisfy instinctive (sexual) needs, the introversive Adler seeks to make man superior to his environment (78 ff.). Schiller contends that modern culture subordinates man to the object, reducing him to a mere instrument of collective advance. This extraversive emphasis in modern society leads to dissociation of thought and feeling in the subject and disturbs the balance and unity of his life. To Jung, however, this lack of balance is due not primarily to external institutions or social pressure, but to our own nature (87 ff.). Nietzsche treats of the same opposition in his contrast of Apollonian and Dionysian; the former has to do with an inner world of phantasy, whereas the other yields himself, with unbridled dynamis, to the instincts which bind him to the group (Seid umschlungen, Millionen) (172-9). In Spitteler's "Prometheus und Epimetheus" the same contrasts appear. Prometheus is the introvert toiling amid his own ideas, while Epimetheus is the conventional man of action with 'correct' ideas, at peace with the world, righteous and self-assured. This extraversive attitude however stifles the highest values; because what once was good does not remain good forever. Changes in racial attitude (*e.g.* religion) are slow and painful; the Epimethean influence persists and cumulates until a true Prometheus comes to kindle for the world a new flame (207-29). In Hindu theology the strife between opposing tendencies is resolved in Brahman

(245); in Chinese philosophy Tao is the middle path which frees man of the struggle with himself (264).

In all these cases, how is deliverance to be gained from the clash of introversive and extraversive forces, of conscious with unconscious? Various methods have been used. The Indian technique is to withdraw attention (libido) from conscious contents; whereupon a kind of hypnotic state ensues wherein the primordial and repressed tendencies of the unconscious are activated and released (250). In western religions deliverance is often symbolized by a divine birth, a god-renewal, a messiah who breaks the shell of collective morality and brings a new way of life (241). Jung finds the way of salvation in *phantasy* or play. Man is only completely man when playing (135). Phantasy is a kind of serious play, wherein the extravert's unconscious introversive tendencies are given release, and likewise the introvert's outlawed extraversive tendencies. From the unconscious springs creative phantasy and religion as well. There is always, between religion and man's actual mode of life, a compensatory relation (174); that is, religion voices the repressed forces which secure no adequate release in ordinary life. Salvation then always begins with the unconscious, that despised region to which is exiled all that is consciously regarded as ugly, immoral, useless; but the dominant conscious forces so abominate these exiled ideas that they crucify the very savior who brings deliverance. "He (the Savior) hath no form nor comeliness—He is despised and rejected of men" (323).

Otto Gross proposes two psychological types, based upon the concept of primary and secondary functions. By a primary function is meant an actual performance of the cells (*e.g.* a representation) which of course dissipates energy; the secondary function is a process of assimilative renewal or restoration. A brief period of restitution implies large capacity for deviation, rapid and superficial adaptability, a flitting from idea to idea; whereas a long secondary function causes the subject to perseverate and form complexes; he becomes intently absorbed with his inner world and may develop excessively valued ideas. The extravert is hampered in unifying his own personality because it is determined by the flux of outer events, while the introvert is hindered by his tendency to form complexes, which break his life into fragments. Intensity of the primary function is the decisive factor for Jung: the introvert has a strong primary response and a correspondingly long period of restitution; the extravert has a weak primary and brief secondary function (337 ff.).

In the field of esthetic appreciation, the extravert finds that the world gains life and value only through his empathic identification with it; whereas the introvert, overpowered by the confusion of the world, seeks esthetic satisfaction in abstract forms (358 ff.).

What James calls the "tender-minded" and "tough-minded" philosophers (372 ff.), and Ostwald's well-known classic and romantic types among men of science (401 ff.), both reduce to forms of introversive and extraversive types.

No critical appraisal of Jung's book will here be attempted. His major ideas are coming into general knowledge and are being reduced to experimental form. The present work is a fruit of much clinical experience and is full of keen observation and illuminating comment. His survey brings into review whole fields of literature which the ordinary psychologist would never touch. Two comments may suffice:

(1) The elaborate symbolism in many parts of the book was much against my palate and little more intelligible than the poems and philosophies which he seeks to elucidate. I am the last to damn a piece of work merely because the writer uses a terminology differing from my own; if a man has something to say I care not whether he puts it in terms of 'libido', or 'gestalt', or 'subvocal speech responses', or 'conscious function,' so long as

I can apprehend what he is trying to say and convert it into my own ways of thinking. Yet I confess being wearied and repelled with page after page of stuff like this: "Just as Prometheus allows all his passion, his whole libido to flow inwards to the soul, to his innermost depths, in complete dedication to his soul's service, his God also pursues his course round and round the pivot of the world, thus spending himself like Prometheus, whose whole being comes near to extinction" (220). It seems too bad that a sentence like that is typical of a book which purports to give scientific information about human nature.

(2) It seems clear that such a dichotomy (introversion and extraversion) has but limited and temporary value, just as would a division of humans into tall and short, bright and dull; a preliminary orientation of this kind must be followed by more careful analysis and exact measures. What with the dominant 'conscious' and the opposed 'unconscious,' in which are confined all the other elements of human nature in repressed or infantile form, the picture of any one type is often pretty confused. Now and then it seems that his theory would fit equally well, were the facts exactly opposed.

These faults however do not crucially impair the value of what his translator calls "Jung's crowning work."

University of Illinois ELMER CULLER

*Mental Growth and Decline.* By H. L. HOLLINGWORTH. New York, D. Appleton and Co., 1927, pp. xii, 396.

The design of this book is ambitiously inclusive. The author sought to provide the college student with a survey text for the whole field of developmental psychology. To bring within the compass of less than four hundred pages the substance of the theoretical and experimental literature on mental growth, the stages of development, hereditary influences, normal variations, abnormalities of growth, characteristics of infancy, of childhood, of adolescence, of mature years, of senescence, and the influence which past generations have left behind them in social institutions was as difficult as it was a commendable undertaking. Psychology is now as much in need of such survey texts in certain fields as the older sciences. All who have tried to teach genetic psychology know full well the problems confronted in attempting to give a class the general scope, the background, and the relationships of the available material on the different stages of the human life cycle. The author has faced these problems and in this volume presents his solutions.

The style is often attractively thought provoking. Instances abound of that kind of sentence which a teacher may use as the basis for developing an instructive class discussion. The chapter on the "post-mortem age" (the survival of institutions) is clever and illuminating. The discussion of maturity as a phase of genetic psychology is a notable and worthy departure from the tendency, far too long current, to think of middle life as a tableland. The last chapter, in which the many laws of development are brought together in clean-cut fashion, is an especially welcome contribution.

If the reader expects to find in this volume an encyclopedic compendium of the material of genetic psychology he will be disappointed. It is not an impartial presentation of the substance of all schools of genetic psychological research. The author has quite obviously selected his material and distributed the emphasis according to his own standard of values. This applies to the factual as well as to the theoretical material. There is a generous use of biological data and of the results obtained by the so-called objective or test methods of trait study. The contributions made by other schools of thought and study receive far less consideration. One looks in vain for the contributions of the *Gestalt* movement to genetic psychology. Köhler's work is omitted and Koffka's is but briefly men-

tioned. If the term 'eidetic' appears in the book the reviewer has missed it. The extensive discussions of genetic psychological problems in the literature of the psychoanalytic school receive but a disproportionate and disparaging consideration. A little of G. Stanley Hall's theorizing appears and some of his clever phrasing, but the vast collections of data which he inspired are scarcely mentioned.

The reviewer believes that many readers will be disappointed in finding no bibilographies, and footnote references are all too few. The teacher who handles this text must himself know the literature very well in order to guide the student to the sources. Fascinating ideas are frequently presented without any reference to lead the interested student into the literature from which they were drawn. But these defects should deter neither the student nor the trained teacher from much use of the book. As an introduction and a conspectus it has no rival in the field of genetic psychology.

University of Oregon EDMUND S. CONKLIN

*An Introduction to Social Psychology.* By L. L. BERNARD. New York, Henry Holt & Co., 1926, pp. x, 651.

Of the recent books in social psychology this volume attempts the most thorough-going systematization of the field which has yet appeared in English. The treatment is divided into four parts: "Introduction;" "The Foundations of Collective Behavior;" "The Integration of the Personality in the Psycho-Social Environment;" and the "Psycho-Social Environment and the Organization of Collective Behavior." The amount of material in this book is so extensive, the treatment so detailed in many places, that the limitations of a review hardly suffice to give more than a passing notion of its contents.

After a preliminary statement of the nature and function of science, especially as it touches the social sciences, the author reviews briefly the field of social psychology, as presented by McDougall, Allport, Williams, Ellwood, Bogardus, Ross, Gault and Dunlap. A working definition of his own is then given:

"Social psychology studies the behavior of individuals in a psychosocial situation. This behavior is valid subject matter for social psychology whether it conditions or is conditioned by other social behavior or responses. It is also concerned with all collective responses, that is, responses of individuals which mutually and reciprocally condition each other and those which are uniform throughout the group, regardless of what environment they arise from."

After this introductory discussion the writer turns to his first task, that of revealing the foundations of social behavior in the individual organism. Taking his cue from the work of C. J. Herrick and C. M. Child, he gives a very able analysis of the mechanisms of man as the highest form of animal life. While he recognizes the part which heredity plays in behavior, he emphasizes, following Child, that hereditary traits are greatly modified and expanded under the omnipresent environment. In the case of man this environment is not merely physical but, all-importantly, it is 'psychosocial,' to use Bernard's own term. The difficulty with the earlier instinct school of social psychologists is uncovered. In this discussion the author draws largely on his own brilliant analysis of the whole theory of instinct in his earlier work. The principal superiority of man lies in the development of the more complex levels of habit formation and of the higher thought processes. The latter he calls the neuro-psychic behavior which finds its higest formulations in the rise of language and symbolic thinking, those all-significant carriers of social behavior. There is also some recognition of the part which so-called unconscious processes play—especially

in reference to repression and the control of deeper patterns of behavior and thought by socially determined consciousness. Thus are the activities of the so-called 'censor' explained. There is some attention to the recent work on intelligence differences, as applied to social classes, nationalities and races. After a rather inadequate review of the current literature on this matter, little further reference is made to this phase of applied psychology.

The next division of the volume takes the individual as an organism and carries it over into the social world in which it becomes a personality. There is a pertinent discussion of the importance of suggestion and imitation as these concern the development of the personality. Imitation, in fact, becomes an important concept for the author. This form of interaction, if it may be called such, ranges from a simple, direct sort of person to person, to the indirect form of imitation of ideal personalities and social standards which are found in the concepts of social values.

The final section treats in detail the nature of the psycho-social environment in which the individual lives, moves and has his being. Primary and derivative (secondary) groups and the types of attitudes and ideals which belong to each sort of group are discussed. There is a competent handling of the problem of social contact and communication. This is followed by a discussion of rational and non-rational groupings. The former are the more deliberate, the more stable forms of organization, the latter of the touch-and-go variety seen in crowds, mobs and temporary organizations of the more spontaneous sort. The relationship of collective behavior to leadership is discussed, showing the place of prestige, propaganda, and other factors. The qualities of leaders, both in primary and in derivative groupings, are analyzed. There is also a chapter on non-institutional controls such as fads, fashions, crazes and transient conventions and beliefs. This is followed by consideration of the place of institutional forms in social control, such items as customs, traditions, conventions, codes and rituals being discussed. The author indicates the growing objectification of institutions and attempts finally to indicate the place which science may play in the modification of institutions, especially in the way of making them more sensible and flexible.

At the close of the volume a splendid and thorough-going bibliography is appended.

What may one say in comment on this very exhaustive treatise? The dual approach of the author is sound. We can not construct a social psychology on the biological behavior of the organism alone: be it in terms of endocrine glands, as with Berman, or in terms of instincts as with McDougall, or be it in terms of individual differences in intelligence as with most present-day educational psychologists. This is only one side of the picture. Bernard has rightly emphasized the selective and all-important place of the environment, especially the social and cultural environment. Some persons may object to the author's invention of new terms, such as 'psycho-social' to describe this environment, and 'neuro-psychic technique' and 'neuro-psychic behavior' to describe the place of cortical functioning. Especially, also, some may object to the extended use of a rather threadbare concept like 'imitation,' especially by a man who wrote a 550-page book to expose the loose use and ill-starred fate of such a term as 'instinct' in the social sciences. Professor Bernard, however, rather carefully tries to avoid the terminology of the elder psychology without laying himself open to the criticisms of either current behaviorism or psychoanalysis. So, too, he attempts to circumscribe the meaning of 'imitation' putting it squarely, as do Humphrey and Allport, in line with the mechanism of conditioned responses. The only danger, as might also be said about the use of the term 'instinct,' is that the usual reader so easily forgets the specific meaning laid down by the author, and holds in mind the

older, looser interpretation of these as sacred entities in the easy explanation of man's social behavior.

All new and uncharted sciences like social psychology or sociology suffer for want of clear, concise terms, largely because the basic concepts in which to describe the phenomena are themselves loose and undetermined. The older phases of both psychology and the social sciences must have patience with the latest arrival in their fields until more adequate observational and experimental data have been acquired. Only then can an adequate, logical vocabulary be established.

University of Wisconsin KIMBALL YOUNG

*Das Recht der Dschagga.* By BRUNO GUTMANN. Munich, C. H. Beck'sche Verlagsbuchhandlung, 1926, pp. 778.

The book represents the seventh publication in the *Arbeiten zur Entwicklungspsychologie* edited by Felix Krueger. Bruno Gutmann, who is also the author of the sixth number (*Amulette und Talismane bei den Dschagganegern am Kilimandscharo*), has already demonstrated his intimate acquaintance with Eastern Africa by several ethnographical and sociological studies. As a missionary he gained in a period of about 20 years, a thorough knowledge of the languages and the history of various ethnological groups in the former German East Africa.

The present work deals with the law of the Dschagga. The Dschagga, a Bantu tribe of about 100,000 people inhabit the southern and southeastern slope of the Kilimanjaro. There are 31 "officially recognized chieftainships" which consist of about 400 independent sibs. In the 69 chapters of his study Gutmann discusses the various legal forms in existence. He devotes the main part of the book to a description of the "norms concerning blood relationships" and the "norms concerning landed property." In the first 17 chapters, 'laws' related to topics of the following kind are discussed: marriage and marriage customs, betrothal, obstacles to marriage, divorce, illegitimate children, minors, responsibility of children, exclusion from the sib, revenge for homicide, and 'blood unions.' In the next 20 chapters there is a very detailed account of legal norms concerning taxes, loans, debts, securities, lost property, markets, canals, and compulsory service. Six chapters are devoted to 'stratification norms,' *i.e.* the rights of the chieftains and of delegates, and the norms regarding plundering expeditions. An additional 6 chapters deal with injuries inflicted on a person's honor, intentional and unintentional killing, theft, and arson. The last part of the book contains an accurate account of the various legal procedures and ordeals.

The present publication is supposed to be a contribution to the *Entwicklungspsychologie des Rechts.* Its lack of methodological, sociological, and psychological discussions is striking. An overwhelming number of facts is recorded with utter care. Nevertheless, Gutmann's study means more than a minute representation of legal norms. Here where the danger of a mere enumeration of 'objective' norms is very great, we find that the author has done more than to present a 'system of law' which in its objectivity is detached from other cultural manifestations. He shows the organic connection of law with religious, social, political, and economical conditions. By talking into account such conditions it becomes possible to trace the genesis of legal norms psychologically. The author does not work with logically simplified 'objectivations' like 'religion,' 'political organization,' 'social structure,' and 'economical life.' He shows in concrete cases in what way these various factors constitute special features in the *Gestalt* of the historically developed life of a group. Thus, an understanding of the genesis and content of legal norms is arrived at by seeing them in relation to other cultural forms. This 'seeing' is not a comparison

of inductively ascertained facts; it is in the case of legal norms nothing but faithful observation of *one* aspect of the *Kulturganze*. The phenomenology of the *Kulturganze* is, however, a prerequisite for the appreciation of the one aspect. We see how legal forms develop by seeing the Dschagga in his relation to disease and death, plants and animals, family and sib. We get a vivid picture of the interplay of the various factors influencing the Dschagga people, but as to the question of 'primary' and 'secondary' factors no philosophical and theological abstractions are made. The legal aspect is represented as one among others. At the same time it becomes clear that the isolated 'individual' to whom legal theories ascribe rights and duties does not exist. Nor does an abstract 'society' exist, but a *Lebensganzheit* with superindividual legal norms of an irrational character.

Undoubtedly monographic studies such as that of Gutmann will be basic for *Entwicklungspsychologie*. We do not get, however, any suggestions from the author as to how the psychological analysis should proceed on such a phenomenological basis. The book itself sufficiently demonstrates the fact that a fruitful analysis has not to start from the 'psychic experiences' of primitives since the individual is not *subjectum agens* of society. In fact, the primitive 'individual' can only be of rather slight importance to the *Entwicklungspsychologie* of law and religion, etc. The fundamental question is, what are the constituents of primitive mentality as they can be singled out in various cultural (legal, mythical, and religious etc.) forms? Detailed analyses—which have, of course, nothing to do with an analysis of the 'psyche' of primitives—have to bring out the typical ways of primitives to 'structurate' the world mentally; analyses which have to show how, in the primitive *Weltbild*, 'objects' are defined and 'attributes' of objects enter various relations, how interrelations between objects are established by the various forms of the causal nexus, and how the proper place of objects is determined by characteristically primitive modes of measuring, numbering, and relating things spatially and temporally. Such detailed psychological (not intrapsychic) analyses on a broad descriptive basis are a desideratum. They are highly valuable for the understanding of the particular character and development of legal, religious, and other forms.

The *Nachwort* by Felix Krueger (pp. 44, which may be obtained separately from the publisher) points out the inadequacy of Wundt's approach in *Das Recht* and stresses the 'organic' point of view in the interpretation of cultural manifestations.

Columbia University HEINRICH KLÜVER

*Lehrbuch der Sprachheilkunde*. By EMIL FRÖSCHELS. Leipzig and Wien, Franz Deuticke, 1925. Revised edition, pp. xxi, 530. 114 figures, 5 plates.

The new edition of Fröschel's book on the art of correcting speech defects is a fuller exposition of his excellent work published in 1912.

It shows a scholarly and exhaustive understanding of the subject and is much needed in a field where there is such a paucity of real scientific writers.

Fröschel treats in general the various classes of speech disorders and is particularly illuminating in explaining the different devices for measuring the musculatures of speech.

His treatment of the disorder commonly known as stammering and stuttering varies from the methods employed by other German writers—particularly Gutzman. He does not go to the extreme in psychology as do the Freudians, nor hold a pathological view of 'nervousness' as is done by many specialists. In discussing the etiology of the subject, he holds that the disorder, being both psychogenetic and psychophysical in its reflexes, it works in a vicious circle of symptoms. Heredity is blamed only

for various anomalies of the cortical speech mechanism. The nucleus of the trouble lies in the psychic condition of the patient, who becomes conscious of the ataxically disturbed speech movements. In the majority of cases there is a disproportion between speaking and thinking. This causes a constant upset, hence a weakening of the syllabary coördination apparatus; in other words, a coördination neurosis.

The generation of sthenic emotions gradually upset the entire speech mechanism and hysterical syndroms appear.

The author suggests three different periods of the disorder. The first stage is that of the single repetition of sounds and syllables between the ages of four and seven. The second period is one of exaggerated, conscious motions of the organs associated with speech, between the ages of six and ten. This stage gives birth to the anxiety neurosis which, of itself, may lead to compulsory movements of the articulatory organs. The third period in the development of the defect occurs any time after the previous stage and may be described as tonic cramp of the articulatory organs and other associated muscles. It is admitted that the neuropathically predisposed child may be more easily subject to such an ataxia and, that, in his case, it will last much longer when further exciting ages arise to upset his speech.

Ithaca, N. Y. FREDERICK MARTIN

*Experimental Psychology: Loose Leaf Laboratory Manual.* By A. T. POFFENBERGER. Chicago, C. H. Stoelting Co., 1926, 48 experiments.

Professor Poffenberger's loose-leaf laboratory manual for experimental psychology is designed for the elementary level. Forty-eight experiments are presented. The author emphasizes experiments that bear upon the learning process. Motor aspects are stressed at every possible opportunity. Observation of behavior assumes first place, although introspective data are not ignored. The order of experiments is left quite elastic so far as any one using the manual is concerned. Among the initial experiments are those on habit formation, practice in puzzle solution, learning to recognize objects, transfer of practice and interference caused by practice. Memory is taken up next. Then come such types of experiments as those on association, imagery, reaction-time, and perception. Experiments are included on aesthetic judgment of form and color harmony, judgment of human character from an examination of the features, judgment of emotional states from facial expression, and muscular fatigue.

This manual appears to have been written with the needs of students of educational psychology primarily in mind. No experiments are included on such topics as perimetry of vision, after-sensations, or color mixture, for example. The manual contains a number of experiments that serve as desirable supplements to the classical experiments in psychology.

University of Vermont PAUL C. SQUIRES

# NOTES

## Some Notes on Behaviorism

The quotations in the present note are taken from Dr. J. B. Watson's article on Behaviorism in Vol. I, of the new volumes of the 13th ed. of the *Encyclopaedia Britannica*, recently published.[1]

To one of the questions proposed by Dr. Watson—"Do we think only in terms of words?"—the present writer feels that he can return a definite answer. He clearly remembers that when he was only about 6 months old he had a certain thought in his mind which he could not put into words at all at the time, but could only translate into action—which he promptly did.[2] The possibility of this recollection being correct and of the episode having occurred objectively at this early age is confirmed by the writer's subsequent observation of the behavior of his little niece. This child had occasionally been given small quantities of honey and at the age of 5½ months she could identify the jar which contained the honey on the table. Whenever it was raised she cried out and displayed very definite and marked signs of wanting some of the contents. This did not apply at all when any other somewhat similar looking object on the table was raised. It seems only reasonable to suppose that the child had some thought in her mind which would have been expressed as "I want some of the stuff inside that object" if she could only have used words at the time. Why should the possibility of thought step in suddenly, and quite adventitiously as it were, just when words are invented, or are capable of being used, and how and why did the first spoken word come to be invented and uttered if the person concerned had had no thought whatever before inventing and using this word? However we need not bother any further about such a fantastic possibility for the writer's clear recollection of his own early thought before he could speak, referred to above, finally disproves it and also finally and completely disproves the behaviorist view that "thinking is merely talking."

Of course it is possible that the use of words to represent things and ideas in a greatly shortened and more concise form may enormously facilitate thought, and extend its range, in the same way that the use of a system of shortened symbols enormously facilitates and extends the range of mathematics. This possibility, however, is not by any means certain although it appears to be so at first sight to adult civilised consciousness. It is quite possible that a savage in a primitive tribe which had not yet invented the use of words might be able to think and act very rapidly and extensively indeed, on certain subjects, without using words at all; and that the same may apply to dogs for instance at the present day.

As long as an organism has a clear idea of the objects involved, words may very likely not be *nearly* so necessary to very rapid, effectual, and complete thought as we speaking adults may often suppose. Of course the utility of words for communicating ideas to other individuals is an entirely different matter. Much adult thinking certainly appears to be in this talking form, and a very great deal of this particular form of thinking is, doubtless, really talking concealed, or repressed, by the operation of the intrapsychial 'censor.'[3]

It can only be lack of knowledge, or the presence of very heavy repressions, which can lead any speaking adult to suppose for a moment that "thinking is merely talking, but talking with concealed musculature." Dr. Watson's idea is so fantastic that the present writer wonders how anybody can hold it even for a moment—it has so clearly placed the cart before the horse. Where does the *talking* come from then? Surely it comes from thought, but admittedly it would appear that it does not always come

[1] *Op. cit.*, 345 ff.
[2] Cf. E. P. Farrow, Eine Kindheitserinnerung aus dem 6. Lebensmonat, *Int. Zsch. f. Psychoanal.*, 12, 1926, 81 ff.
[3] E. P. Farrow, A method of self-analysis, *Brit. J. Med. Psychol.*, 5, 1925, 118.

from clear thought amongst the behaviorists. In their case it would appear that the production of a large amount of talking, and presumably of writing and literature as well, is not preceded by the slightest amount of thought for their own statement of their position quoted above is to this effect. Can it be wondered at, then, that they produce some curious literature?

It would be very interesting to know exactly how and why the behaviorists come to ignore the striking phenomena of thought and consciousness in the world. "The Behaviorist takes the position at the outset that the total behavior of man from infancy to death is the subject matter of (human) psychology. Behavior can be observed like the phenomena of all other natural sciences, *e.g.* chemistry, physics, physiology, or biology. The same general types of methods used in the natural sciences can be used in behavior psychology. So far in his objective study of man no behaviorist has observed anything that he can call consciousness, sensation, perception, imagery or will. Not finding these so-called mental processes in his observations, he has reached the conclusion that all such terms can be dropped out of the description of man's activity."

Doubtless "the behavior of man is the subject matter of (human) psychology," but this is a very different matter from saying that it is the *whole* of the subject matter of human psychology. Dr. Watson seems to imply the latter proposition; but he apparently dare not say so explicitly for if he did it is clear that nobody would agree with him. It is obvious that something very important would be excluded. It is doubtless true that "so far, in his objective study of man, no behaviorist has observed anything that he can call consciousness, sensation, perception, imagery or will." That is not to say, however, that these latter things do not exist and can therefore be ignored. While we cannot directly observe another person's consciousness, he can inform us of his various states of consciousness, and of his various thoughts; and we can observe (*i.e.* record) all his various statements about these. Such statements are objective scientific phenomena in relation to a behaviorist observer. Why should they, therefore, be ignored?

There seems to be a craze for applying the methods of one objective science in the department of another. Admittedly a biologist who is used to describing, and endeavoring to understand, objects which he can see and often actually handle is apt to be very suspicious of the electrons and protons of the physicists, for example, which nobody has actually seen, and whose existence is merely inferred from certain effects on evidence more or less incomplete. The biologist may have a shrewd and unwelcome suspicion that, one day, much of the present electron theory may possibly go by the board in much the same way that the former necessity for a luminiferous ether has suffered badly under the theory of relativity. Even in that event, the present stage of atomic theory would doubtless have served a useful purpose in leading to the discovery of something else.

Admitting all this, and admitting that consciousness in other beings cannot be directly observed as a biologist would like to do, yet he is directly aware of his own consciousness and thinking powers, and the objective behavior of other people corresponds in certain respects so very closely with his own behavior that he is absolutely forced to attribute to them a similar consciousness with similar, or greater, thinking powers. While the biologist is directly aware of his own consciousness and power of thought which are not inferences at all but are, on the other hand, the instruments with which he infers, the attribution of consciousness to other beings is admittedly pure inference. It is, however, so much the simplest explanation of which the biologist can think to account for the observed phenomena that, while it is admittedly an inference, it is nevertheless an inference to which even a biologist is reluctantly completely forced by the overwhelming weight of evidence.

This ignoring of the striking phenomena of consciousness and thought in the world is so remarkable that one can only think that the condition of mind which can do this must be of a pathological nature. It would be of value and interest to know exactly how such a particular pathological attitude of mind can arise and something about the structure and aetiology of the pathology. The matter is of great practical importance as well, for great harm may be done by the behaviorists if they attempt to put their theories into practice without realising the consciousness and capacity for suffering which other human beings and animals possess.

An injurious effect of this kind may often be produced by many of the behaviorists' experiments on small children. Suppose a child is given a 'conditioned reflex' of fear of a fish-bowl by showing it the fish-bowl and then striking a steel bar behind its head. It does not by any means follow that when the child's 'fear reflex' of the fish-bowl is 'deconditioned' by bringing the fish-bowl gradually up to the child while it is eating that therefore the harm caused to the child by frightening it with the steel bar in the first place is thereby removed from its mind, in the way Dr. Watson seems to imagine. Some of these particular experiments with small children may do far more harm than the experimenters realise. It is far too readily assumed that every so-called 'reflex' in a small child is an 'unconscious' phenomenon.

The serious and fundamental error or omission associated with the behaviorist views may also be of great practical importance from a national point of view if they ever came to be applied to the construction of an army, or to the conduct of industrial affairs. Fortunately, however, there is probably extremely little likelihood of this. There is good reason to suppose that an army constructed and organised internally solely on the inhuman and artificial behaviorist conception of humanity would immediately break up and disintegrate completely on receiving severe blows from an enemy. On the other hand, in the case of an army organised largely on the conscious and affective phenomena of patriotism and mutual affection, blows from an enemy only serve to weld it into a more compact and homogenous whole.

In industrial affairs too, the ignoring of the phenomena of consciousness and suffering would doubtless produce great disaffection and probably strikes as well. The latter, however, would probably be regarded as 'behavior' and might lead to a revision of the theory.

With regard, however, to the aetiology of the pathology of the peculiar behaviorist view in the minds of the behaviorists the present writer has no very definite evidence. It would seem to him that perhaps a narcissistic phenomenon (a blind believed following of 'a formal method of other sciences') may play a large part in the pathological structure. This, however, would hardly account for the extraordinary blinding to the wonderful fact of consciousness. For an explanation of this striking phenomenon one would expect to find something far more powerful in the minds of the behaviorists.

Nevertheless we cannot hope to know anything very definite about the pathology of their peculiar views until several behaviorists have been analysed. It is thus greatly to be hoped, in the interests of science and the future happiness of humanity, that some rabid member of the creed may in the near future have his mind investigated, straightened out, and 'put right,' by means of the penetrating analytic process.

A form of wish fulfillment may often enter—namely a strong "wish that human psychology could be so greatly simplified in this way." We certainly should not, however, allow such forms of wish-fulfillment processes to blind us to the otherwise obvious phenomena of external reality.

Apart from some of their scientifically very interesting observations and experiments upon the 'conditioned reflex' and other subjects which

must, after all—although they may modestly deny it—have required a considerable amount of thought for their preparation, one must give the behaviorists credit for having realised that a very large amount of the ordinary adult form of thinking is really concealed talking. Many other people indeed, including the present writer, had, however, realised this a long time ago by observational work upon their own minds. Dr. Watson seems also to have realised by direct observation the important fact that 'taps' in infancy have a very important bearing indeed upon this concealing process, a fact which most psycho-analysts do not yet seem to have realised at all by means of following the analytic process.[4] The present writer realised the very great strength and importance of this particular mechanism in the case of his own mind two years ago by means of self-analysis.[5] Dr. Watson, however, does not seem to have a sufficiently detailed knowledge of the precise nature of the psychological and instinctive mechanisms at work in this particular repressive process, and he probably does not realise at all how extensively and profoundly the taps which he was himself given in infancy affect him at the present day, in the same way that such taps similarly affect every other individual who has not been deeply analysed. Clearly one thing which is the matter with the behaviorists is that they do not realise, from direct knowledge, what happens in the individual human mind before the individual is able to talk, and that they have no knowledge as to how the concealing process of this early talking, and of the thoughts which underlie and produce this early talking, is brought about. If they had this knowledge, they could not possibly hold the behaviorist view. The early simple form of thinking is enormously more primitive than the later acquirement of mere talking.

With regard to the question "Does thinking itself offer any problem?" the present writer can only say that, to him, at least, this question is absurd for it is obvious that it contains many problems which may possibly not be completely solved for thousands of years and perhaps, indeed, never. The early thoughts of a human individual, before he is able to speak, when viewed by means of subsequent psychoanalysis, are apparently largely concerned with such things as a desire to fulfill certain deep-seated longings, and the avoidance of those circumstances which produce fear. For example, the writer's early thought referred to above was concerned with violent opposition towards a certain interference with the feeding process. Psychoanalysis shows that the influences of these repressed early thoughts enter as factors or components into practically all the forms of later adult thinking. There appears to be no discontinuity about the process, and indeed one would not expect any such discontinuity in a biological matter of this kind. Adult forms of thinking are thus closely bound up with the satisfaction of instinctive desires in infancy. In their turn, the instinctive desires, as we understand them, doubtless go back a very long way in the vertebrate scale. Man is not to be separated off sharply from the rest of the animal kingdom in the way the egocentric behaviorists would have us believe by supposing that thinking does not offer any problem and that it is "merely talking." The results of certain experiments indicate that even frogs can think, and are possibly conscious, in a very simple way under certain circumstances—that is to say that this is much the simplest explanation of the frog's very peculiar behavior in the course of the experiments. In this connection, it may be pointed out that the essence of sound science is simplicity. The simplest explanations which can be found in our endeavor to account for all the observed phenomena are the ones which should naturally always be selected.

Spalding, England E. PICKWORTH FARROW

[4]Cf. E. P. Farrow, On the psychological importance of blows and taps in early infancy, *Psychoanal. Rev.*

[5]*Op. cit.*, *Brit. J. Med. Psychol.*, 5, 1925, 118.

## The Present Status of Psychology in China

Psychology as a natural science in China gives much promise.[1] At present, it is inseparable from education and in that connection its achievement is marvelous. "The coming of Dr. McCall to China as the director of Psychological Research in the National Association for the Advancement of Education in 1923 has given to the movement of tests and measurements in China a center of impulse and guidance that has already yielded unexpected results."[2] The famous "TBCF Scale System" and some useful inventions or discoveries that came out incidental to the scaling and standardization are the results.

"This scale system is a sort of eclectic one, representing as it does an attempt to embody as many as possible of the best features of the other leading systems, namely, grade scale, percentile scale, and age scale."[3] The four test units were named in honor of those whose contribution to scientific mental measurement has been of the most fundamental significance. T (a unit for measuring the total amount of a pupil's ability in some trait) is for Thorndike, the originator and teacher of scientific educational measurement and author of the first College Entrance Intelligence Tests, and for Terman, the author of the Stanford Revision of the Binet-Simon Scale and leading exponent of the age-scale system. B (a unit for measuring a pupil's rate of progress and hence his *brightness* in the trait in question) is for Binet, the creator, with Simon, of the first Intelligence Scale, and for Buckingham, the creator of the grade-scale system. C (a unit which automatically indicates a pupil's *classification* in a standard school in China) is for Courtis, an early pioneer in educational measurement and originator of practice tests; for Cattell, who, with Fullerton, laid the foundation built upon by Hillegas in constructing the first statistically satisfactory judgment scale; and in remembrance of CHINA where this unit was first devised and used as such. F (a unit for measuring *effort* or how much the pupil has learned in proportion to his native intelligence or capacity to learn) is for Franzen, Pintner, and Monroe, all of whom published, at about the same time, a practical mechanism for measuring achievement as related to capacity to achieve. This unit is used only when both an intelligence and an educational test have been given. All intelligence and educational tests were constructed, scaled, and standardized according to this system.

Some of the useful inventions and discoveries incidental to the scaling and standardization of the tests are: (1) a simple, generalized formula allowing for chance regardless of the number of alternatives; (2) a generalized formula for the inclusion of elements in alternatives such that full knowledge will be required before correct answers can be given; (3) a new method for constructing product scales such as composition and handwriting scales; (4) a method for the construction of a C scale for an automatic reading of a pupil's classification in a standard school; (5) a straight line equation for all C scales; (6) a simple and identical logarithmic equation for all B scales; (7) a new method for the construction of intelligence tests; and (8) a new and unusually simple formula for measuring the efficiency of a school and of a school system.

Articles appearing in *The Chinese Journal of Psychology*, *The Chinese Educational Review*, *Chung Hua Educational Review*, *Elementary Education*, *Secondary Education*, *The New Education*, *The Eastern Miscellany*, and *Science* are almost exclusively on tests, measurements and statistical treatments, although some original articles on child study are not infre-

[1] At the request of Professor Washburn, the author writes this note at the expense of competency. It is his wish that news of the work of Chinese psychologists may henceforth be furnished directly from China.

[2] W. A. McCall, Scientific measurement and related studies in Chinese education, *Bull. Chinese Nat. Assn. Advancement Educ.*, 7, 1923, 2 ff. Foreword by P. C. Chang.

[3] *Ibid.*, 6. Cf. also McCall's *How to Experiment in Education*, 1923.

quently met. There are a few experimental works on reading. *The Chinese Journal of Psychology*, which has four volumes now, is owned by the Chinese Psychological Society.[4] Professor Y. C. Chang is the president of the Society and, at the same time, the editor of the Journal. The National Association for the Advancement of Education has a section on Eductional Psychology, from which all achievements in that field come.

Psychology departments are found in Yen-Chin University, National University of Peking, Peking National Normal University, Peking Women's College in Peking; Nankai University in Tientsin; Teachers College in Wuchang; National Southeastern University in Nanking; Fu-Tan University in Shanghai; Amoy University in Amoy; and National University of Kwangtung in Canton. Although most, if not all, of them have some apparatus for demonstration and elementary laboratory courses, none has yet a regular laboratory building for research in experimental or animal work. Fu-Tan University in Shanghai is said to have built a Psychology Laboratory for Mr. Zing Yang Kuo, formerly of the University of California, but it is doubtful whether any research is carried on there.

To be a psychologist in China now is to be an all-round psychologist. While giving courses in general, educational, experimental, social, child, abnormal and comparative psychology, one is still entitled and required to devise tests, both 'intelligence' and 'educational.' Some of the earlier psychologists are T. C. Chen, Y. Tang (both of Cornell), T. T. Lew, Y. C. Chang, and H. C. Chen, all of Columbia. Mr. H. C. Chen and Mr. Ping Lin (the latter formerly of Stanford) are the only specialists on Child Psychology. Mr. Kong Wu was a student of William James. Mr. T. L. Wong did some important animal work in Chicago but has now quitted that field. Mr. C. W. Luh's research on "The Conditions of Retention" was considered by Professor Carr to be one of the best Ph.D. dissertations from the Chicago Laboratory. Mr. Luh is now recognized as one of the foremost, if not the foremost psychologist in China. Mr. Z. Y. Kuo started explicitly the anti-instinct movement in this country by his sensational article, "Giving Up Instincts in Psychology." Mr. T. C. Tu did work on vertical and horizontal reading of Chinese in the University of Iowa, and Mr. Hua I, formerly of George Washington University, is now performing some experiments on reading, using prisoners as subjects. Mr. Eugene Shen, a statistician, logician, and psychologist, who did important experimental work on the reading of Chinese by photographically recording eye movements at Stanford, is still in this country.

There appeared last year in the *Eastern Miscellany* two articles introducing the *Gestalt* theory. Some of the books translated into Chinese during the last ten or fifteen years and some of the original treatises on psychology published comparatively recently are as follows:

(1) *General Psychology*: Dewey, How We Think; Hart, Psychology of Insanity; Höffding, Outlines of Psychology; Poffenberger, Applied Psychology; Watson, Psychology from the Standpoint of a Behaviorist; Woodworth, Dynamic Psychology; Wundt, Introduction to Psychology.

(2) *Child Psychology*: Gaupp, Psychologie des Kindes; Kirkpatrick, Fundamentals of Child Study; Tracy, Psychology of Adolescence.

(3) *Educational Psychology*: Colvin, The Learning Process, Human Behavior; Edwards, The Fundamental Principles of Learning and Study; James, Talks to Teachers on Psychology; McCall, How to Experiment in Education, How to Measure in Education; Starch, Experiments in Educational Psychology; Thorndike, Educational Psychology (Briefer Course); Whipple, How to Study Effectively.

(4) *Social Psychology*: Ellwood, An Introduction to Social Psychology; LeBon, The Crowd, La Psychologie politique et la defense sociale,

[4] Cf. this JOURNAL, 38, 1927, 487 f.

Les opinions et les croyances, Psychology of Revolution; McDougall, An Introduction to Social Psychology; Marbe, Grundzüge der forensischen Psychologie; Wallas, The Great Society, Human Nature in Politics.

(5) *Original Treatises*: Cheng, T. C., Outlines of Psychology (probably the oldest text); Kuo, Z. Y., Human Behavior, Vol. I (extreme behavioristic treatment); Luh, C. M., Psychology (the only textbook for the Senior Middle School), A Treatise on Social Psychology; Liao, S. C., Educational Psychology.

Stanford University — SIEGEN K. CHOU

## THE ACCURACY OF THE METHOD OF CONSTANT STIMULI

Culler[1] in his recent note on the method of constant stimuli has found that, in the fractionated data of Urban's[2] lifted weight experiments, there is a correlation between c and h, and on the basis of this concludes that these two variables are not independent as Urban stated, but are dependent. All of Culler's correlations are worked out in terms of $\rho$ (Spearman's rank-correlation). If these had been calculated in terms of r (Pearson's product-moment correlation) there would have been considerable difference from the values which he has given. Table I gives some values of r and $\rho$ calculated from various experiments where the method of constant stimuli was used. The first seven of these are from lifted-weight experiments and the remainder are in the visual-perceptual field. In the case of lifted-weights there is in general a high positive correlation between

TABLE I

LIFTED-WEIGHT EXPERIMENT

| $S_1$ | | $S_2$ | | |
|---|---|---|---|---|
| r | $\rho$ | r | $\rho$ | |
| .998 | 1.000 | .999 | .943 | Fernberger[3] |
| .973 | 1.000 | 1.000 | .943 | Fernberger[4] |
| .987 | .996 | .996 | .996 | Fernberger[5] |
| .984 | .996 | .991 | .991 | Fernberger[6] |
| .997 | .750 | .850 | 1.000 | Fernberger[7] |
| −.352 | −.620 | .828 | .250 | Fernberger[8] |
| .999 | 1.000 | .999 | 1.000 | Fernberger[9] |

VISUAL PERCEPTUAL EXPERIMENTS

| S | | | S | | |
|---|---|---|---|---|---|
| r | $\rho$ | | r | $\rho$ | |
| .912 | 1.000 | | .362 | .500 | |
| .184 | .500 | Fernberger[10] | .243 | .900 | Oberly[12] |
| .419 | .500 | | .164 | .000 | |
| .996 | 1.000 | | .902 | .900 | |
| .073 | .500 | Fernberger[11] | .957 | 1.000 | Oberly[13] |
| .986 | 1.000 | | .600 | .900 | |

[1] Elmer Culler, The accuracy of the method of constant stimuli, this JOURNAL, 38, 1927, 307-312.

[2] F. M. Urban, Der Einfluss der Übung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271-311.

[3] S. W. Fernberger, Table I, The influence of mental and physical work in the formation of judgments in lifted weight experiments, *J. Exper. Psychol.*, 1, 1916, 515.

[4] Table II, *ibid.*

[5] Table XI, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 14, 1913, no. 61, 39.

[6] Table XII, *ibid.*, 40.

[7] Table I, An experimental study of the 'stimulus error,' *J. Exper. Psychol.*, 4, 1921, 71.

[8] Table II, *ibid.*

[9] Table III, *ibid.*

[10] Table VI, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 130.

[11] Table VII, *ibid.*

[12] H. S. Oberly, Table VII, The range of visual attention, cognition and apprehension, *ibid.*, 35, 1924, 342.

[13] Table VIII, *ibid.*

c and h although in one case there is a low negative one. In the visual-perceptual field there is a decided tendency towards a low correlation. Where there is a low correlation the value of $\rho$ tends to be greater than r, in one case a difference of 0.657.

From the statistical theory of sampling we should be led to expect that c and h would remain fairly constant for a given observer, unless practice entered or there was a change of attitude on the part of the observer, but theoretically it is not clear what a high correlation between these would indicate, perhaps homogeniety of material or relationship between the variables.

Further if, as Culler contends, c and h are not independent but dependent, then the method of Least Squares breaks down, for our observation equations would take on the form

$$\begin{aligned} hx_1 - F(h) - \gamma_1 &= O \\ hx_2 - F(h) - \gamma_2 &= O \\ \cdots\cdots\cdots\cdots & \\ hx_n - F(h) - \gamma_n &= O \end{aligned}$$

where

$$F(h) = c$$

or we would have

$$\begin{aligned} f(h, x_1, \gamma_1) &= O \\ f(h, x_2, \gamma_2) &= O \\ \cdots\cdots\cdots & \\ f(h, x_n, \gamma_n) &= O \end{aligned}$$

and

$$\begin{aligned} h &= G(x_1, \gamma_1) \\ h &= G(x_2, \gamma_2) \\ \cdots\cdots & \\ h &= G(x_n, \gamma_n) \end{aligned}$$

so that the most probable value of h might be either the arithmetic or the geometric mean.

Since the $\Phi(\gamma)$-hypothesis is directly dependent on the method of Least Squares it would seem that if there is dependence between c and h the whole method would have to be rejected. Further if c and h are dependent then has Culler not used quantities which were obtained in an improbable manner to establish this relationship?

University of Pennsylvania HAROLD M. LUFKIN

## TWO PRONOUNCED CASES OF VERBAL IMAGERY

A number of years ago (1914) I discovered, among my students in a class in experimental psychology at the University of Oregon, a girl (M. M.) whose imagery seemed to be limited to the verbal type. During the past year, I discovered a man (W.A.), among my students in elementary psychology, whose imagery type seemed similarly limited. These two cases are so marked and unusual that I think it advisable to make them a matter of record.

M.M. was of the pure verbal-motor type.[1] The heard word had meaning to her only as she repeated it. She never took notes during a lecture, for she found it impossible to write and at the same time to repeat the lecturer's words. She had to devote her entire effort to the repetition of the words as they fell from the lecturer's lips. She even had to repeat the words spoken to her in a conversation if she wished to understand what was said. When she failed to pronounce a word, it was as if that word had been spoken in a foreign tongue.

[1]For a description of another case of this type cf. S. W. Fernberger, Possible effects of the imaginal type of the subject on aphasic disturbances, this JOURNAL, 30, 1919, 330 f.

Tests that I gave her showed that she comprehended the spoken word, just as the average reader comprehends the written word, by way of throat kinaesthesis. In other words, she 'heard' as the average person 'reads.' In one of the tests I had her protrude her tongue and hold it between her teeth, so as to inhibit in a measure the verbal movements accompanying the heard word, and then I read sentences to her—which fell within the range of her memory span—at various rates of speed. At the moderate and slow rates she was able to reproduce the sentences, but at the rapid rate she failed. In another test I read a sentence to her while she was rapidly repeating a word aloud. Under these conditions she either failed to comprehend what was read or her rate of repetition slowed down noticeably.

As might have been expected, she read kinaesthetically; and for a motor reader she read at an extremely rapid rate. It is possible that the practice effect in 'motor hearing' carried over to the reading.

M.M. was sensitive regarding her 'defect,' as she looked upon it. She had long been aware that she differed from other people, having first realized it soon after she started to grammar school; but until she took psychology she did not know just how she differed.

As far as I was able to determine, she lacked visual and auditory imagery and was of the pure verbal-motor type. If she was under a handicap because of this, she was successful in overcoming it. Her university record showed no failures, and she was graduated with her class with marks above the average.

W.A.'s images were verbal like M.M.'s, but of the auditory type. Unless the 'heard' or 'read' words were accompanied by auditory imagery they were meaningless symbols to him. He comprehended conversations and lectures, or words read or written, through auditory verbal images. The 'word' to be understood, had to be imaginally 'heard.' Even W.A.'s thinking was done to a great extent in auditory-verbal images.

The knowledge that he differed from other people came to him, as to M.M., at an early age. He says, in a carefully prepared report on his type of imagery, that "I first noticed that my reading differed from the average person's when my cousin commented on the fact that she could read much faster to herself than out loud. As I had never noticed any difference in speed between oral and silent reading I tested it and found that there was no difference for me. I was about 9 yrs. old at the time."

W.A.'s imaginal type did not handicap him in grammar school because he had sufficient time to cover small assignments, but as he advanced to the higher grades, to high school, and to college, it became increasingly difficult for him to do the required work. He went through lectures, recitations, and hours of study under a constant surge of auditory-verbal images, which were at times as vivid as hallucinations. "The perpetual sounds of words in my head," he writes, "produced headaches, which became more and more severe and frequent as the number of lectures and the length of the class-room assignments increased."

In his senior year in high school W.A. made, at the suggestion of his teacher in classics who became interested in his case, a serious attempt to change, or at least to supplement, his type of imagery. He tried to read visually, but with no success. "Verbs, prepositions, adjectives, and adverbs," he reports, "meant absolutely nothing to me, and only a few of the nouns and pronouns carried meaning." If he was to graduate, he had to cover his assignments; consequently his attempt to modify his imagery was short-lived. The only effect of the attempt was to intensify his feeling of difference.

He found, when he came to Cornell, that the assignments were too long to be done by his usual methods of study, and so he appealed to his class

advisor—a member of the department of psychology—for assistance. At the suggestion of his advisor he again attempted to alter his type of imagery. He counted aloud so as to drown the sounds of his auditory images, and at the same time he attempted to visualize as he read. It was of no use, however, for he could not understand what he read in this way. Though he made an earnest effort, the attempt to modify his imagery again failed.

The din of the auditory images which always accompanied his verbal perceptions became so intense that he seldom passed a day without a headache. The headaches became progressively more frequent and more severe until—just before he decided to leave college—he found it impossible to study for more than half an hour at a time.

W.A.'s images were almost exclusively of the auditory-verbal type. Visual images were rare in his experience, and when they did occur they were fleeting and unclear. I was unable to tell whether he had motor images; if he did, they did not bulk large in his mental life. He localized the auditory-verbal images within his head, and described them as high in pitch.

Cornell University KARL M. DALLENBACH

## MR. GUNDLACH ON "SOME DIFFICULTIES WITH WEISS'S BEHAVIORISTIC POSTULATES"

I do not suppose that Mr. Gundlach expects me to meet his criticism of my postulates.[1] However, a general statement of my own reason for not doing so may interest the readers of this JOURNAL. If Mr. Gundlach's point of view is adequately expressed by his quotation from Bentley (IIC); it (psychology) "seeks to describe and to understand experience and the activities of the total organism in which experience plays an essential part," and if my point of view is expressed by 'psychology studies the development of those movements, from infancy to maturity, which make the individual a coöperative unit in a social organization' then the futility of our trying to reach any agreement seems obvious. For me it is immaterial whether or not (IIC) "psychology has its own definite problems and point of view which are not to be confused with those of other sciences." On the contrary I hope that some day psychology will be a part of Science with a capital S, as Max Meyer has recently expressed it. Nor do I see any more objection to making "psychology the handmaid of the social sciences" than the mathematicians object to having mathematics regarded as the handmaid of all Science. On the other hand, the traditional psychology which Mr. Gundlach represents does insist upon "its own definite problems and point of view" and does object to the 'handmaid' principle. We also differ widely on the principles of criticism. Mr. Gundlach avoids "subjecting himself" to what he calls "the criticism of internal inconsistency which less explicit authors happily escape." This is the atmosphere of clever debating, not that of science. As one of the "too explicit authors" I prefer not to engage in a debate. It seems to me that a most eloquent refutation of my postulates would be the formulation of an alternative set. This would leave the scientific reader free to chose between them.

Ohio State University A. P. WEISS

## A REJOINDER

Through the courtesy of Professor Weiss and of the Editors of the JOURNAL I have been given an opportunity of appending a rejoinder to his 'reply,' but there is no need nor occasion for me to say anything as Professor Weiss does not touch upon my criticisms. He merely states why he does not reply. Criticisms, however, are not met by ignoring them, nor are internal inconsistencies justified by a refusal "to engage in a debate."

University of Illinois RALPH GUNDLACH

[1] Ralph Gundlach, this JOURNAL, 38, 1927, 469-475.

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